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## ORIGINAL ARTICLES.

### **A SERIOUS FALLACY ATTENDING THE EMPLOYMENT OF CERTAIN DELICATE TESTS FOR THE DETECTION OF SERUM-ALBUMIN IN THE URINE, ESPECIALLY THE TRICHLOR-ACETIC ACID TEST.<sup>1</sup>**

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THERE is no point in medicine about which there is more universal disagreement and misapprehension than that relating to the absolute and relative value of recently introduced delicate tests for albumin in the urine. Certain it is that in the hands of any save those fully conscious of the many fallacies attending the use of these tests, and with disbelief in the existence of a normal albumina minima, it is far better to depend upon the less misleading, if also less delicate, and more practical time-honored methods—those by boiling<sup>2</sup> and by the use of nitric acid. Although certain complications attend the use of these, they are more easily overcome and are less likely to mislead than those surrounding such tests as trichloroacetic acid, the reagents of Tanret, Sebelein, and Millard, glacial phosphoric and picric acids. Sources of error are so numerous with most of the latter tests that our best urinologists strongly question their practical clinical utility. Saundby, for instance, remarks<sup>3</sup> that "there are albuminoid bodies in the urine that have no known clinical significance, and these can only embarrass the practitioner who finds them when he is looking for a sign to which he attributes a significance based upon purely empirical foundation." Saundby discards all tests save that by boiling with a drop or two of dilute acetic acid. Many others, among whom is our own distinguished Tyson, hold very similar opinions, and view with small favor the substitution for the heat and the nitric-acid tests the more recent aspirants for favor. The many fallacies attending a reaction from these delicate tests are sufficiently dwelt upon in systematic treatises on urine-testing; for example, the presence in

the urine of albumoses, peptone, drugs previously ingested, such as an alkaloid or a coal-tar derivative, an excess of urates and the like—all may at times confuse.

One source of error but little noticed, to which I wish now to direct especial attention, is that relating to the presence in normal urine of an albuminous body or bodies allied to serum-albumin, the so-called nucleo-albumin, the precise chemical nature of which is unfortunately little understood. Much that has been written in the past regarding the presence normally of traces of serum-albumin in the urine, as ascertained by the employment of most refined tests for its detection, is valueless, because of disregard of the fact of the existence of this substance in most urines in amount sufficient to respond to most of the delicate tests used for serum-albumin, and from which the reactions could not be distinguished. Such observers were either unaware of or disregarded the fact that nucleo-albumin<sup>1</sup> derived from the extra-renal urinary passages might readily have caused the response to the tests employed for the detection of traces of serum-albumin.<sup>2</sup> This substance is normally present in the urine of all in varying amount. The quantity, which is usually infinitesimal, though generally sufficient to react without concentration of urine to certain tests used for the detection of serum-albumin—notably such as the trichloroacetic acid, the reagents of Tanret, and also Sebelein—may increase in conditions of irritation of the urinary passages induced by muscular exercise, or by the presence of a urine too acid and irritating (concentrated) to an amount to cause even doubt as to its differentiation by Heller's nitric-acid-contact method, a test regarded as a very certain one for the detection of serum-albumin.<sup>3</sup>

<sup>1</sup> There seems doubt whether or not urinary nucleo-albumin is a single body and identical with mucin. At all events it possesses a remarkable similarity to the nucleo-albumin of the bile, the basis of biliary mucin. Neubauer and Vogel fully discuss these points in their splendid work on the urine.

<sup>2</sup> Posner's researches as to a physiologic albuminuria are faulty in this particular, as are those of Capitan, Chateaubourg, and several others.

<sup>3</sup> Reissner, by estimation, found urinary mucin present to an amount varying from 0.05 to 0.1 per cent., and v. Noorden noted as much as 0.6 gm. to a liter of normal urine. (See Neubauer and Vogel, Wiesbaden, 1890.) Kirk, in a most admirable paper on "Urine-testing for Mucin" (*Lancet*, April 26 and May 3, 1890), gives instructive data as to the differentiation of mucin from serum-albumin. Kirk regards urinary mucin as a substance occupying an intermediate place between other forms of mucin and albumin. The first-mentioned authorities, than which there

<sup>1</sup> Read as part of a discussion before the Society for Clinical Research, March 27, 1894.

<sup>2</sup> Applied to the upper stratum of clear acid urine in a test-tube, to be viewed with a light from above and a dark background for comparison with the unboiled stratum.

<sup>3</sup> Bright's Disease, p. 125. Bristol, 1889.

In the thoroughly healthy, with urine normal as regards nitrogenous ingredients and free from ordinary indications of catarrhal conditions of the urinary passages, it is not unusual to obtain a trace of response from contact for from a few moments to a half-hour with a cold picric-acid solution, and I have found it common to obtain an almost instantaneous fine white ring by the other tests mentioned, the trichloroacetic acid and that of Sebelein especially, but also by that of Tanret; and, less often, by that of Millard. Glacial phosphoric acid also reacts after from three to five minutes' contact. The presence of mucin in these same urines, if they be not too rich in salts,<sup>1</sup> may often be readily shown by the use of acetic acid as a precipitant. My own urine almost habitually responds to several of these very delicate tests, such as trichloroacetic acid, to the reagents of Tanret and of Sebelein, and often to picric acid and to Millard's reagent, though it is absolutely normal as regards urea and freedom from casts. This response is probably due to the presence of mucin, perhaps derived from the bladder, which has a tendency toward irritability. I have often treated specimens with acetic acid after the manner detailed in the papers on "Non-albuminuric Nephritis," and have then always failed to obtain response with the treated specimen on subsequent testing with picric-acid. The test of Tanret, and the trichloroacetic acid test, then, also, occasionally tried for experiment, would still not unfrequently give a response, but not to any extent the sharp reaction previously obtained.

I have in the past so repeatedly obtained a reaction to these tests in urines which I had with good reason regarded as normal that I long ago ceased to place any confidence in a positive response unless first, among other sources of error, the presence of this urinary mucin could be excluded. Unfortunately, this is impossible as regards several, and especially that of trichloroacetic acid, that now most in vogue as a delicate test for albumin.<sup>2</sup> This acid, from its

possessing properties similar to acetic acid, of which it is a substitution-derivative, I have become convinced is an absolutely uncertain and even dangerous reagent in this particular, so likely is it to mislead. As is well known to those accustomed to urinary analysis, it is an impossibility to separate traces of a mucinous substance from the urine even by the use of acetic acid with prolonged contact, though the latter be added in sufficient amount to overcome the neutralizing effects of the urinary salts;<sup>1</sup> and even should all be precipitated by long contact, which is doubtful, if it be present in any amount, as is so common with specimens of the diurnal urine, its complete separation on the filter is impossible. Insoluble substances, such as chalk, magnesia, barium sulphate, added to assist its precipitation, will also carry down traces of serum-albumin if naturally present or artificially added. I have, however, found again and again that with urines which I was convinced contained no serum-albumin, but which responded to picric acid and more markedly to the trichloroacetic-acid contact-test, sufficient mucin could be removed with acetic acid to prevent further trace of response to the former, but not to the latter.

It is for these reasons that, though I have for the past eight years employed the unacidulated picric-acid test as a routine method, first overlaying the suspected urine with it before proceeding to the application of the heat or of other tests, I have placed far more confidence in a negative than in a positive result obtained by it. A reaction not occurring within a few moments, the employment of other tests was, naturally, regarded as unnecessary. A slight response appearing within this time, and not attributable to the other sources of error before mentioned, I had, until recently, attributed to traces of serum-albumin, and was inclined to believe in the existence of a normal slight albuminuria. More lately, however, experiments have convinced me that this response is due to the presence in the urine of a mucinous body.

To ascertain the frequency of reaction to the more delicate tests for albumin, of urines from normal individuals, those so far as was known that were without indications of renal disease, I recently examined specimens from 105 young men, all of whom save one regarded themselves in good health at the time, and but three of whom were known to be taking drugs of any sort. It was certain that none was a gonorrheal subject. The

from serum-albumin. The best of the refined tests are the plain saturated solution of picric acid and Millard's solution. Although acetic acid enters into the composition of the latter, it is partly as an acetate, the action of which, as a precipitant of mucin, is slight. Yet this solution, too, reacts with mucin, as its originator acknowledged.

<sup>1</sup> One-tenth bulk of glacial, or its proportion of 25 per cent. acid.

are no more exact, do not look upon this body as true mucin, but call it nucleo-albumin, or *mucinähnliche Substanz*, as do, indeed, also Hammersten (Physiological Chemistry) and others of our best authorities.

<sup>2</sup> The urinary salts maintain mucin in solution. Concentrated urines, *i. e.*, those rich in salts, will not yield mucin to a small amount of acid, save by special treatment. In diluted or in dialyzed urines, or in those naturally poor in saline ingredients, the presence of mucin may be usually detected by the addition of a small amount of dilute acetic acid, and allowing to stand, care being taken not to add too much after dialysis. Some urines, from presumably normal individuals, cloud markedly almost instantly on the addition of an excess of the acid.

<sup>3</sup> Other of the delicate tests, such as those of Tanret, Sebelein, etc., containing free acetic acid, and also a mercurial salt, or tannin, are also unreliable, from their readiness to respond to traces of mucin, not at all in the dusty contact-haze that many advocating the use of these tests teach, but often as a sharply-defined ring, the appearance of which cannot be distinguished

specimens were collected for me by my friend Dr. Wolff, in the course of laboratory instruction, and in a manner that admitted of no deception being practised by the donors. The urethra was first flushed with urine which was discarded, so that mucus from this situation might be excluded.

The specimens were divided into two sets, one of 52, the other of 53. All of the 105 urines were those of the day. All save three of the first series were voided about three hours after breakfast, and as with the second series, which specimens were passed about four hours succeeding dinner, after the donors had been standing at laboratory work at least three-quarters of an hour. All of these specimens, therefore, represented the diurnal urine and food urine, in which the erect posture and evidences of muscular fatigue would aggravate an albuminuria if already present, and would likewise tend to induce an increase of mucinous substance in the urine such as would be similarly present in a mixed twenty-four-hour specimen from the same individuals.<sup>1</sup>

In the first series (52) three of the commonly-used delicate tests were employed—saturated solution of plain picric acid, trichloroacetic acid, and glacial (meta)phosphoric acid.<sup>2</sup> In the second series (53), trichloroacetic acid was the only one of the three tests employed.

For convenience, positive or negative response to the tests employed was noted at the end of three periods, and marked as occurring within these: ten seconds (which in the application of the contact-tests by picric acid and by trichloroacetic acid was practically instantaneous), one minute, and five minutes.

In the first series the trichloroacetic acid used was a specimen of Merck's C. P.,<sup>3</sup> which, originally crystalline, had liquefied after nearly two years' standing on the laboratory shelf, through the slow absorption of moisture from the air, the glass stopper of the bottle not having hermetically closed the neck. No water whatever had been added. The specimen, therefore, represented a highly-concen-

trated solution. I had previously found that the delicacy of reaction to the acid depended very greatly upon the degree of concentration of the solution. A solution which at one time I regarded as quite saturated, to which water had been added and no heat employed to insure solution, was noticed to react with no more delicacy than did Millard's solution or metaphosphoric acid. This fact is of importance, and probably explains the variation in result obtained by different experimenters with the acid.

In the second set of examinations, a freshly-obtained specimen<sup>1</sup> (labelled Kahlbaum, Berlin) of the crystalline acid was used, which was first liquefied in a water-bath and fluidity maintained by the addition of a small quantity of a concentrated, but not completely saturated, solution of the acid (Merck's C. P.). There was only sufficient of the first solution remaining for one comparative test with the last. By it the latter solution was found to not quite equal the former in delicacy of response, the reaction appearing somewhat later, and being less decided.

An unacidulated, saturated solution of picric acid was used. This and the trichloroacetic acid were applied by the contact-method, viewed with a dark background by a light from above. The technique in examinations of this sort is of the greatest importance. It should be stated that as a background a piece of black cloth was always used. Most of the examinations were made at night, by aid of gaslight. When by day, illumination by sunlight was, of course, avoided. Inclination of the *upper part* of the tube toward one, with proper shading often demonstrated with the trichloroacetic acid, a sharp, white, undoubted, though fine ring, that otherwise would have been overlooked in the contact-time mentioned. Tubes of three-eighths-inch diameter were used in applying the trichloroacetic acid, the cost of the acid not permitting too large a quantity to be employed in each test. The urine was first added to the tube, and the acid subsequently deposited at the bottom with a pipet drawn out to a very capillary extremity.

In testing with picric acid the urine was overlaid. With the metaphosphoric acid, a small, thoroughly clean and clear portion of a cylinder was dropped into the urine, and the time of development of the slightest cloud about it noted within the time-limits stated. All the specimens tested were undecomposed. A small quantity of dilute acetic acid was added to all that were not decidedly acid. Any specimens that were at all cloudy were filtered.<sup>2</sup> With but three exceptions all of the 105

<sup>1</sup> This was not intentional. Morning specimens would have been preferred if obtainable without chance of deception. It is, however, of value, for the reasons stated.

<sup>2</sup> Picric acid was chosen as the test I most frequently employ; trichloroacetic acid as one now coming much into vogue, and with which I had been disappointed in the past, as with Tanret's and Sebelein's reagents, from the too extreme delicacy misleading, as already stated. Millard's was not now employed, as I was satisfied from some comparative tests with it in the past that it corresponded very nearly in point of delicacy and exactness with picric acid. Tanret's solution was not used, as experiments had shown that it is a less delicate reagent for the detection of mucin or serum-albumin than trichloroacetic acid. Metaphosphoric acid was used, as doubt existed as to its relative delicacy with the others employed.

<sup>3</sup> Given me by Dr. Leffmann. He had opened the bottle occasionally to remove a few crystals, using them and not a solution in his testing with it.

<sup>1</sup> From the Chemical Laboratory of the Jefferson Medical College, through Dr. Lawrence Wolff.

<sup>2</sup> Swedish filtering-paper was used. This is important, as the



specimens gave an undoubted response<sup>1</sup> to the trichloroacetic-acid solutions, prepared as indicated, in the shape of a contact-ring within the five-minute limit. This reaction varied from a very narrow, always white, ring in the least marked to one of many lines in thickness in the most decided. Fifty-nine responded within ten seconds; of these, 22 reacted very markedly instantaneously;<sup>2</sup> 10 others by a sharp, though very thin white ring, becoming, as with nearly all, broader with longer contact. The remainder of the 59 reacted at first slightly, though undoubtedly the line became sharper with from one to five minutes' contact; 34 of the remaining responded within one minute, and of these, 13 only reacted slightly within the minute, becoming, however, except in two instances, more marked at the expiration of five minutes. Nine only of the 102 reacting showed no response at the end of a minute, the precipitate forming within the five-minute limit.

The three that gave no response save a trace of haze within the five minutes—the only ones of the 105 that did not unquestionably react—showed a pronounced contact-cloud of from three to four lines in depth within a few seconds after placing the tubes in hot water. The influence of heat, applied in all instances and most satisfactorily by placing the tube in a vessel containing water at the boiling-point, was found without exception to render the previously-obtained reaction more decided, developing in certain of them a contact-cloud instantly, which was almost a minute in appearing in a second tube not subjected to heat. So that trichloroacetic acid seems a test the delicacy of which is even intensified by the application of heat.

With picric acid, 41 of the 52 tested responded within five minutes; 16 reacted within ten seconds; 7 of these 16 responded very markedly instantly (these were all albuminous). Four others of the 16 gave a sharp, but very narrow ring. The reaction in the remainder of the 16 (5) was a mere contact-haze, which in 3 deepened into a sharply-defined, though narrow ring, within one minute, the remaining 2

within five minutes. Thirteen of the 52 that did not respond in ten seconds reacted within the minute; of which 13, 2 showed a hazy contact-ring only, not increasing at the expiration of five minutes. Six others displayed a fine, sharply defined, though not broad ring, which had considerably increased at the expiration of five minutes. In the remainder of these the ring was better defined, though not broad. Of the 23 remaining of the 52 which gave no response at the end of one minute, 11 showed no trace of haze within five minutes, while the 12 reacted within this time. In 6 of these 12 the reaction amounted to a haze only; the remaining 6 gave a sharply-defined though narrow ring. Of the 11 giving no reaction to picric acid at the end of five minutes, all gave a precipitate, as has been stated, with trichloroacetic acid—5 within one minute, 4 within five minutes, the remainder within ten seconds. In this same 11, metaphosphoric acid gave absolutely no reaction in 6; in the others, the response with the glacial acid was very slight or questionable (at five-minute contact).

With metaphosphoric acid, 10 of the 52 tested failed to show signs of cloud in five minutes, and 36 in one minute; 4 only responded at the end of a few seconds, and these were of those showing albumin by nitric acid. Twelve reacted within one minute that did not within ten seconds; of these, 7 responded very slightly and 5 markedly. Of the 5, 2 gave the HNO<sub>3</sub> contact-reaction. Ten of the 36 that failed to show a haze within one minute also gave no response within the five minutes. Of the remaining 26, 23 had reacted slightly at the end of five minutes; the other 3 had responded markedly.

The influence of heat on the reaction obtained with trichloroacetic acid and with picric acid, applied in all cases after the occurrence of the response noted, showed that the precipitate was not due to urates or to a proteose or peptone. In several of these urines, other than those responding to coarser tests, there was a decided mucous precipitate, though the urines filtered perfectly clear. The action of acetic acid was tried on only one of these—this specimen was voided about four hours after dinner by a man with whom I am thrown much in contact and whom I know to be in the best of physical condition. This urine gave a slight ten-second response to trichloroacetic acid; at the end of one minute the faint, white ring had become decided. A haze was shown in ten seconds' contact with picric acid. This, at the end of a minute, had also become a sharp ring. With metaphosphoric acid the reaction was but slight at the end of five minutes. With the nitric acid contact-test, at a distance of about four lines above the meeting-line of acid and urine, there occurred a cloud three or four

French gray paper contains, and will yield to the filtrate, sufficient vegetable albumin to cause response to the finer tests, as Millard pointed out.

<sup>1</sup> As regards comparative time of response, it is interesting that with the first series (of 52) the reaction appeared within ten seconds in 41, of which 6 were markedly serum-albuminous; while in the second set tested (53), with a different solution of trichloroacetic acid, as already detailed, but 18 reacted within the ten seconds, of which 14 were also markedly albuminous. Of the first series, 2 only of the 52 failed to respond within the one-minute limit. These 2, however, reacted within the five minutes. Of course, this wide dissimilarity in time of reaction may be due to differences in the character of the urines, instead of in the degree of concentration of the two test-solutions.

<sup>2</sup> In 20 of these the reaction was so instantaneous and very decided as to leave no doubt as to the presence of serum-albumin in some amount, afterward confirmed by other tests.



lines in depth, unaffected by heat.<sup>1</sup> At the junction of the two fluids a marked chromogen ring was so decided as to obscure any slight reaction which might then be produced by traces of albumin; 5 c.c. of 25 per cent. glacial acetic acid added to 15 c.c. of this cold urine filtered, which was perfectly free from a trace of cloud, gave after standing a few moments a very marked smoky haze, unaffected by gentle heat, indicating a decidedly mucinous condition of the urine. It was impossible to separate by repeated filtration this precipitate, so that no further tests were proceeded with.

Though this interrogation was not undertaken for the purpose of ascertaining the frequency of occurrence of so-called "physiologic" albuminuria, it is of interest here to note that of 105 urines from adolescents and young men, all save one of whom regarded themselves as in very good health, 20 (19 per cent.) gave an undoubted serum-albumin reaction to nitric acid by the contact-method. The percentage of albumin in these urines, subsequently estimated by Esbach's method in nearly all, lay from one-fourth to one and one-fifth grams to the liter. I have carefully examined, physically,<sup>2</sup> all save one of the 20. No one of the 19 show evidences of renal disease. In nearly all the albuminuria is intermittent, and is probably the result of circulatory disturbances, due to the erect posture and a weak heart. In these albumin is absent from the urine on rising, but present in that after exercise during the day; and pulse-tension, ascertained by the sphygmograph as well as by the finger, is low, in several markedly so.

Excluding these cases of undoubted albuminuria, the results obtained indicate with little question either that, normally, urine contains traces of serum-albumin, or, more probably, that the response to trichloracetic acid and that occurring less frequently to picric and metaphosphoric acid was due to the presence of a body of nucleo-albuminous (or mucinous) character of extra-renal origin. The results obtained with these 83 urines reacting to trichloracetic acid and not to the coarse tests<sup>3</sup> were so constant and agree so perfectly with those I had obtained in prior examinations of urine from those

with presumably healthy kidneys that I have no doubt as to the validity of this inference.

To my mind these results show conclusively that as a practicable *delicate* test<sup>1</sup> for albumin, the much vaunted trichloracetic acid is of little value. Responding as it does so universally, its utility is much less than that of metaphosphoric and especially that of picric acid, the last of which not only furnishes useful negative information, but also, from its less refined delicacy, positive results of value, excluding reactions to a few drugs when present in the urine,<sup>2</sup> a precipitate from urates, the proteoses or peptone,<sup>3</sup> and mucin or allied bodies, all readily differentiated, except the latter if in excess.<sup>4</sup>

With trichloracetic acid, unless the response be frank and outspoken—i. e., unquestionable to tests less delicate but reliable, such as ebullition with the addition of a few drops of dilute acetic acid, than which there is none less likely to err; those tests, in other words, that do not so readily react to urinary mucin—the nature of the result must remain in doubt and cannot be utilized to account for the symptoms if nephritis be suspected, as it is, perhaps, more than likely not due to serum-albumin.

From the results here obtained and from those I have before published, with those of many other observers, notably Kirk, and those who preceded

<sup>1</sup> In a note concerning the test, by Reese of Johns Hopkins Hospital (*J. H. H. Bulletin*, February, 1890), it was regarded as one worthy of extended use in clinical work, because of its utility and delicacy. It was narrated that many cases had been encountered the urine of which responded to it (used in saturated solution), and not to control-tests, such as picric acid—that frequent source of error, urinary mucin, or an extra-renal albuminous body, not being regarded in question. In a number of these cases (eleven) granular, epithelial, and hyaline casts were found in the urine, and a subsequent necropsy showed distinct changes in the kidneys. This is of especial interest to me, indicating as it does the frequency of non-albuminuric nephritis; for in this category these cases must certainly be placed. The so-general response of the urine of the healthy to this test, and often to picric acid as I have shown, indicates beyond doubt in my mind that in the cases referred to by Dr. Reese, the trichloracetic-acid reaction was but a coincidence, obtainable markedly in as many consecutive healthy subjects.

[Since writing the foregoing I have learned that trichloracetic acid has ceased to be regarded with its former favor in this hospital, especially by Prof. Kelly, for the reason stated—its impracticable delicacy. Prof. Osler has recently written me that he is "rapidly coming to the conclusion that about one-half of the chronic degenerative changes in the kidneys are unaccompanied by albumin in the urine. The word comes over so constantly from the laboratory '*degenerative changes in the kidneys*' in patients whose urine showed no especial alterations," Prof. Osler has begun to think that "there are no normal kidneys in hospital patients above forty years of age."]

<sup>2</sup> Which, if in no other way, may be obviated by the withdrawal of medicines for the time.

<sup>3</sup> None of which is often obtained, but all of which may be recognized by placing the tube for a short time in hot water, the heat of which will immediately greatly diminish or dissipate the response.

<sup>4</sup> By removal with acetic acid in the manner I shall detail in a future communication.

<sup>1</sup> This reaction, so often met with in both albuminous and non-albuminous urines, has been shown by Kirk to be due to mucin (*Lancet*, May 3, 1890), as was first suggested by Roberts (*Medical Chronicle*, October, 1884, p. 1.)

<sup>2</sup> Time has not yet permitted microscopic examination of more than two of these urines. One of these men has a weak heart; he is an overtrained athlete. A second has a mitral systolic, and a third an aortic diastolic murmur. In neither of these two, however, are there other indications of back-working apparent. Compensation seems perfect.

<sup>3</sup> The contact nitric-acid test, supplemented by heat, was made with all urines responding in a suspiciously decided manner to trichloracetic acid.

him on similar lines, but who, unlike him, regarded the frequency of response in the healthy to refined tests as indicative of a normal, constant albumina minima, it may be finally concluded that the recognition by such reagents as trichloroacetic acid of mere traces in the urine of what the most skeptical regard as albumin, even with symptoms suggestive of renal disturbance, can have little significance, because of the very general frequency of reaction of urines of normal individuals to the same test. If this reaction be due to serum-albumin, its significance is actually *nil*, as it would follow that a normal, slight albuminuria is common. It is, however, more probably, in the vast majority of instances, due to the presence of an albuminous body of extra-renal origin, a nucleo-albumin, or so-called mucin. In certain other cases—pathologic subjects, with symptoms indicative of a type of chronic nephritis, in which serum-albumin, as ascertained by more reliable tests, is persistently absent from the urine—it may, and probably does, also arise from the epithelium of the tubules of the kidney,<sup>1</sup> due to a faulty metamorphosis of the same producing a renal mucinuria.<sup>2</sup> Its recognition in the latter, in which, for purposes of diagnosis, its differentiation from serum-albumin would be unnecessary, if further differentiation from the common extra-renal mucinuria could be arrived at, as by obtaining urine directly from ureter or kidney, would be of the highest value. But as the latter is a clinical impossibility, we must fall back upon our only actual reliable aids in the diagnosis of such renal suspects—the microscope and the ureometer—the examination of the measured twenty-four-hour urine for urea and its sediment for casts.

I am greatly indebted to Dr. L. Wolff, and his assistants Drs. Jacob and Strecker, for the trouble taken in obtaining the series of specimens of students' urine for examination; and to Professor H. Leffmann and Dr. G. P. Thomas, for great assistance in the examinations. In all the specimens examined, save some six, all reactions that appeared were carefully noted and timed by one of the latter of these, as well as by myself.

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The Medico-Legal Society of New York will hold its May meeting at the Academy of Medicine on the 9th of May, 1894, at 8 P. M. The following papers will be read: "Medical Witnesses," by H. W. Mitchell, M.D.; "Railways and Railway Surgeons," by Clark Bell, Esq. The meetings are open to all persons interested in the science.

<sup>1</sup> See remarks as to the presence of renal mucin in the urine in such cases, in the two papers on non-albuminuric nephritis, in the *Amer. Journ. Med. Sciences*, December, 1893, and THE MEDICAL NEWS, April 14, 1894.

<sup>2</sup> Associated usually with a cylindruria.

## THE ELEMENT OF MALIGNANCY IN TUMORS.

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THE cause or causes giving rise to the element recognized as "malignancy" in tumors has so far been only a bone of contention among pathologists and surgeons, without their ever having been able to present any distinct reason or theory which could seem to merit the general approval of able observers. Probably that view advanced by Cohnheim, namely, the factor of "reduced physiologic resistance" has met with a more hearty acceptance than have any of the so-called "blood-theories" that preceded it, and, so far as I am aware, it is the first so-called local explanation for malignancy that has been put forth.

The theories to be presented in this paper are partly a rearrangement of already existing views and partly the suggestion of some new ideas, so far as I am aware, now brought forward for the first time. Prior to the subject proper, there are some views to be considered that have been generally accepted as facts: 1. Tumors consisting of embryonic tissue afford in the highest degree the best example of malignancy. 2. The more purely cellular the tumor, the richer in bloodvessels and the less developed their coats, the more rapid its spread and the earlier and more certain its generalization. 3. The more nearly that a tumor represents adult histologic elements, the less the degree of malignancy, *e. g.*, the spindle-celled sarcomata are less malignant than the round-celled variety. 4. That there are marked blood-changes not in accordance with the local or general disturbance produced by the development of a neoplasm, cachexia and anemia occurring without ulceration or pain, and many times not at all in proportion to the mildness of these symptoms.

From the existing knowledge of embryonic tissue, it is to be presumed that it can be produced and its growth will be more rapid when the nutrition is lower than that required by adult tissues; it is also known that its growth is far more rapid in the embryo prior to that stage in embryonic life in which specialization of tissue occurs. There are many reasons for inferring that embryonic tissue does not require for its growth a nutritive value so high as do the adult tissues. It is a well-known fact that fractures in patients the subjects of cachexia producing alterations in the nutritive value of their blood are abundantly surrounded by a growth of embryonic tissue, while the organization of the new or inflammatory products does not take place,

<sup>1</sup> Read before the Philadelphia Pathological Society, January 15, 1894.

and hence the fractures do not unite with usual promptness. This condition occurs not only in subjects that are the bearers of malignant tumors, but in general blood-diseases, and more especially in those in whom the tendency is toward the lowering of the bodily nutrition. In long-standing suppuration, when the general body-nutrition is bad and adult tissue wasting, how often do we find the embryonic tissue growing exuberantly? So much is this tendency respected that surgeons often decline to operate when the cachexia is well marked. If then, we admit that embryonic tissue can and does reproduce itself when adult tissues cannot retain their vitality, then it remains to be supposed that the embryonic growth or neoplasm removes from the blood nutritive elements not yet elaborated for the adult tissues, and thus saps them of their nutrition prior to its full development. That such elements exist in the blood cannot be doubted if we believe that the bone-marrow, the lymphatic glands, and the spleen have anything to do with the elaboration of the blood.

The second fact with regard to the cellular elements of a tumor is partly defined in the preceding remarks upon the growth of embryonic tissue; but I do not think that sufficient attention has been given to the condition of the bloodvessels in new growths. In the malignant tumors, at least, these are almost devoid of walls, so much so that in some of the rapidly growing sarcomata they are no longer blood-vessels but are cavernous sluice-ways, irregular in their conformation and anastomosing at every point without the formality of contracting their caliber to anything like what might be called a capillary. These large blood-ways are not endowed with any contractility, and no vasomotor system is to be demonstrated either histologically or clinically; hence their caliber cannot be affected, and the quantity of blood which is supplied to a neoplasm of this class is always the same and not altered by the nervous modifications which are applied to the adult tissues. We should also remember that no part of the nutritive material utilized in the growth of neoplasms is for the production of force; neoplasms have no function, no conversion of food into energy is required, and hence the entire pabulum supplied is converted into cellular growth. In glands a certain amount of the food utilized is represented by secretion, but in glandular carcinomata there is no secretion.

With regard to the third point referred to, namely, that the more nearly a malignant growth represents an adult element, the less malignant it becomes, again bears out the blood-theory of its nutrition. It has now reached a point that its growth calls for the same degree of nutritive value that the adult

tissue does, and when it can grow the adult tissue can survive; and often when such a tumor, a sarcoma of course, is found to suddenly spring into activity, upon removal it will be demonstrated that it has reverted from a large spindle-celled to a small spindle-celled, or even to a round-celled growth, and with this will be found the beginning emaciation already referred to. This sudden conversion of a spindle-celled tumor, growing slowly, into a round-celled tissue growing rapidly, I have seen demonstrated again and again, and at each change in cellular elements the general health showed the change after a very short interval. If a simple adult-tissue tumor becomes an embryonic-tissue neoplasm we have all noticed how rapidly cachexia follows, and we have also noticed how slowly cachexia comes on in the simple adult neoplasms, in which it is rare and occurs not from the growth itself, but as a rule from interference with other tissues and their function.

As to the last point in the consideration of this view of the malignancy of neoplasms, namely, the change in the blood itself, there are many elements that have not been duly considered. With the exception of a few Continental observers there have been few observations recorded upon the blood of patients suffering from malignant neoplasms. Freund, of Vienna, asserts that in carcinoma the blood contains sugar, and in sarcoma no sugar, but only peptone—a view confirmed by Andrews. Upon this I have not been able to make any observation, but after numerous observations upon cases of malignant neoplasms I can assert that the red and colorless blood-corpuscles are diminished and the coloring-matter reduced, in many cases lower even than in many of the marked cases of anemia. I recall one case occurring in the practice of Dr. Hearn, of this city, in which an enormous round-celled sarcoma of the breast, of rapid growth, not associated with any ulceration or pain, but only with the discomfort of its weight, had emaciated the woman to almost a skeleton. The tumor was removed at the request of the patient, and before she left the hospital she had gained considerably in weight, in the face of so severe an operative procedure. This gain continued until recurrence became well marked; then retrograde change evinced itself, and rapid emaciation again followed; a second operation was followed by a second improvement in the general nutrition, and once more a second recurrence was followed by as rapid a decline in the bodily nutrition. This fact of general improvement after operations in the most grave cases in which any operative interference seems debatable, may be well advanced for palliative operations, and the improvement seems to show that the removal of the growth at once modifies the general blood-condition, and the resulting improve-



ment of the bodily nutrition bears out the theory already advanced. Given a patient with a malignant tumor of even short duration, and tell twenty surgeons who have experience with tumors that it is a tumor, and they will from the face diagnosticate that it is malignant without seeing it, and many of them will venture its classification from the general appearances. I would again assert that these cachexiæ are often entirely out of proportion to the general symptoms and lesions which are found, and, contrary to much of the teaching are not, in many cases at least, associated with either pain, ulceration, or visceral metastasis. Besides, who has not seen a woman going about and looking well upon whose leg there was a chronic ulcer, probably attended with varicose veins, the suppurating surface two or three times as large as the area of ulceration of almost any carcinoma of the breast and productive of many times the degree of suffering. The cachexiæ of malignant tumors are not as a rule due to, although they may be intensified by, hemorrhage, pain, mechanical effects (*e. g.*, pressure, weight, etc.), and visceral metastasis, which but represents a new center of growth, and aids in sapping the system. Again, if the tumor required the same quality of pabulum as the adult tissue, why should it not waste when the adult tissues waste? Unless it be from some of the reasons already cited, the cause is yet to be sought.

#### THE ETIOLOGY, PATHOLOGY, AND TREATMENT OF OZENA.

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THE term ozena is a most unfortunate one, for the condition is not a disease *per se*, but a symptom indicative of a number of pathologic tissue-changes with altered secretions. This condition, then, may occur in various inflammatory conditions of the nasal mucosa, as well as in ulcerative conditions, syphilis, malignant growths, retention of secretions from polypi, rhinoliths, and foreign bodies.

The atrophic change is the one to which this term is most commonly applied and upon which it really depends, and when considering the pathology and treatment we must, therefore, study the pathologic changes on which this condition depends.

The differences of opinion as to the cause of the disease furnish conclusive evidence that there is room for further investigation. In my opinion, it is subsequent to the so-called hypertrophic rhinitis. The mucous membrane consists of a basement-membrane upon which are arranged epithelial cells; underneath are bloodvessels, glands, and nerves, which are

imbedded within the interstitial submucous tissue; its essential function is to secrete mucus.

Inflammatory conditions, with engorged vessels, causing intra-vascular pressure, are followed by exudation, which relieves the intra-vascular pressure, but which in turn brings about extra-vascular pressure or interstitial infiltration.

If this inflammatory material organize there will be interstitial thickening followed by contraction, but in either case nutrition will be interfered with, causing a certain amount of atrophy, with consequent degeneration and alterations dependent upon the extent of nutritive change. This alteration of tissue interferes with normal glandular secretion and elaboration, so that the material elaborated is more highly albuminous and slightly more fibrinous, rendering it highly coagulable; hence the tendency to the formation in the sinuosities of "crusts" or "slugs."

Herzfeld, of Berlin, has been able to demonstrate the presence of muscular fiber even in the Schneiderian membrane, and in this statement he has been confirmed by Kölliker and Zuckerkandl. Voltolini, however, failed to find such fiber.

If we grant that thickening and contraction do not take place, the atrophy can be explained in the same manner as red atrophy of the liver. When the mucosa is once passively congested it tends to remain so, and by continued intra-vascular pressure the interstitial tissue atrophies and degenerates. When the submucosa and cavernous tissue are cyanotic the epithelial lining dependent upon them for nutrition must necessarily undergo degenerative changes. The infiltrated cells, together with the proliferated, fixed cells, may from poor nutrition undergo granular change.<sup>1</sup> The transition from hypertrophy to atrophy does not necessarily imply the presence of ozena; in some cases with widespread atrophy, and in which the nasal capacity had greatly increased, no odor could be detected. According to Zuckerkandl, hypertrophy and atrophy are more pronounced in the respiratory region, yet by no means limited to that area, but not infrequently invade the olfactory area. This condition may exist in one or both nostrils. The condition of unusual size of the nostrils is more a result than a cause, dependent upon the amount of contraction and atrophy. In many cases there is narrowing of the rhino-pharynx; in some cases adenoid vegetations form an important element, and in such cases the rhino-pharynx should be carefully treated.

<sup>1</sup> The examination of sections made from tissue removed showed in three cases the thickened interstitial tissue, while in two cases the atrophy was due to pressure from engorged vessels. In each case the tissue was examined within twenty-four hours after removal.

Much has been written in regard to the effect of bacteria in catarrhal conditions. The normal nasal secretion may possess anti-microbic properties, but such is not the case with inflammatory exudates. That there is any etiologic connection between bacteria and the disease remains to be proved. In my own experience I have been able to isolate many germs from inoculations made from the diseased exudate in which marked odor existed. Some are pathogenic, others non-pathogenic, but against any one germ being an important factor is the fact that no one germ was always present. In most cases in which the odor was pronounced I found present a germ which by macroscopic and microscopic appearance, as well as by the test of *sense of smell*, was undoubtedly the bacillus *fœtidus*. It may be noted that Löwenberg, Christovitch, and Fraenkel believe in the microbial pathogenesis.

The history generally given by the patient is that the trouble began in a series of "colds in the head," followed by marked anterior and posterior nasal discharge. As the case progresses the character of this discharge alters from a thin, slightly colored discharge to a thick, purulent one, and continuing so for some months, with slight exacerbation, only to return with increased severity, the discharge becoming more albuminous, with a tendency of the secretions to dry, or "crust," and form "slugs" in the sinuosities of the fossæ, with some little odor noticeable. This condition goes on from bad to worse. The patient suffers from frontal headache, which is increased on stooping; sharp neuralgic pain over the malar prominence, or "face-ache;" itching and burning of the eyeballs; irritability and depression; constant itching and burning in the nares, due to the presence of altered secretions, and by constant blowing of the nostrils the condition is only aggravated. On swallowing, the patient experiences that peculiar sensation of fulness in the palate-region due to the accumulation and retention of secretions in the post-nasal space. Usually the general health is impaired and the patient is anemic.

As to anemia, almost all cases exhibit it in which there is interference with nasal breathing. This has been clearly proved by a series of blood-examinations of cases in which there was nasal obstruction; counts being made before and after relief. Usually only two or three counts can be made after treatment, but in all cases the count shows a rapid increase in the number of corpuscles per cubic millimeter, and not only an increase in number, but the shape of the corpuscles, from being markedly irregular and crenated, becomes normal in appearance. My attention was first directed to this fact by Dr. Sajous, and through his suggestion I was led to make the investigations.

Zaufal observes that fetor of the breath is most marked in young subjects, and that after twenty it is less. In this I do not agree, as in some of the cases which came under my observation the patients were over thirty, and if the odor was then *less*, the preëxisting condition must have been pitiable. Surely those whose specialty necessitates their sitting in front of a patient afflicted with ozena will agree that it is one of the most offensive of conditions, and that its cure means to the patient what no one can realize except one thus afflicted.

During the hypertrophic stage of the disease small nodular masses are discharged, described by the patient as pieces of flesh. Microscopically, these masses are usually polypoid or adenoid; their presence is indicative of tissue-destruction.

The plan of treatment from which I have been able to obtain the best results is: (1) Thorough spraying of the membrane with some solvent of albuminous material; (2) washing away of this material by means of an alkaline antiseptic solution; (3) thorough drying of the membrane; (4) the application of the remedial agent, best in a solution, which will adhere to the membrane. After cleansing and drying the membrane the next step is to apply a stimulating, irritating substance which will establish an acute inflammation of the parts. This is simply applying to the mucous membrane the general surgical methods of treating chronic superficial inflammations by converting them into acute inflammations. In this way one will produce hyperemia of the parts, deplete the circulation by increased activity of the blood-current, and lessen the engorgement. This end is also furthered by transudation, which, being on a mucous surface, has free drainage.

When a copious watery discharge has existed for at least ten days then the cleansing and drying should be followed by a stimulating antiseptic solution. In cases in which the atrophic changes have been followed by degeneration the benefit derived from treatment is very slight except so far as relates to the lessening of the odor.

As a solvent for the albuminous material, hydrogen dioxid (15 vol.) is possibly the best. To wash away this material an alkaline solution of sodium bichlorate and bicarbonate with cinnamon-water and camphor-water, or any of the alkaline washes, is good. After carefully drying the membrane, the parts can be stimulated and irritated by applying oil of mustard in benzoinol or liquid albolin by means of an atomizer. As an irritant it should be used in some cases as strong as six or eight drops to the ounce.

If the condition is not too far advanced this plan of treatment has, in my hands, been most successful, the formation of slugs being entirely relieved, as well as the dryness and itching of the nares, the

fulness of the post-nasal space, the secretion becoming almost normal. The patient's general health must, of course, be improved by tonics, regular out-of-door exercise, and rest.

1632 CHESTNUT STREET.

## CLINICAL LECTURE.

### PULMONARY TUBERCULOSIS.

*A Clinical Lecture  
delivered at the Philadelphia Hospital.*

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GENTLEMEN: I have brought this array of patients before you to-day in order to preach a sermon. I want to preach to you the gospel of hope in the treatment of tuberculosis.

We cannot say that patients in this hospital are subjected to the most favorable conditions for the treatment of pulmonary tuberculosis; we do the best for them that circumstances will permit; and we do much better for them than could be done by their relatives and friends at home. But we are not able to command all that we would like in the way of nutritive and other therapeutic measures. Notwithstanding this, we are still able to keep a large number of our patients comfortable, and to prolong their lives for considerable periods.

The patient to whom I first call your attention—Henry D.—is aged fifty years, and has been in this hospital about five years. When you read in the newspapers that somebody or other, perhaps a so-called physician, for many quacks have "regular" diplomas, and of all quackery the quackery of a physician fallen from grace is the worst—when you read that such a one has invented or discovered some wonderful remedy for consumption, and that he has given it to one or two or four thousand patients, and that in ten minutes or ten days they have all been cured, just remember that this patient has been here for five years and, though he is kept very comfortable and has frequently for many months together been without cough or night-sweats or febrile temperature, enough improvement, therefore, to build several newspaper "cures" upon, he is not well yet. Cases of pulmonary tuberculosis have, even when left to themselves, an average duration of life of some four years. Those, therefore, who talk about "curing" pulmonary tuberculosis by novel methods and who report cases of "cure" under anything less than five years, are either unfamiliar with the natural history of the disease or, for reasons sufficient to themselves, do not make use of their knowledge. In a few months, or even in one or two years, nothing can be said as to the ultimate issue of the case; and I want to impress this upon your minds, because I do not want you to be misled, either by quack things, like the "Amick Cure," or semi-quack things, as was the "Koch Cure" in its incipency, into the belief that there is a "specific" against tuberculosis, or by the unfortunate example of Koch and his followers into premature announcement

or premature adoption of any line of treatment. Six years at least must be allowed to elapse after the institution of any particular treatment in an ordinary case of pulmonary tuberculosis before one is entitled to say whether or not it has been curative. We may, of course, say in any reasonable time, in a few months if circumstances warrant, and more especially if the treatment has stood the test of years in other cases, that it has kept a patient comfortable, that it has increased his strength, that it has apparently augmented his chances of recovery; and this word—recovery—is the one that I would like you to use rather than *cure*; because *cure* implies that something brought in from the outside has conquered the disease, and *recovery* means that something within the patient has wrought a change in his condition which has brought the morbid processes to an end. We have to aim, in the treatment of this disease, at recovery, to be brought about not by "specifics," not by miracles, but by various measures, principally hygienic, which put the patient into such condition that the exciting causes of the disease are powerless to harm him. It is the *vis medicatrix naturæ* which limits and finally brings to an end the pathologic processes, and the physician's function is to stimulate that inherent tendency to recovery, to assist its action, to remove the obstacles that hinder its success.

The theory which I hold as to the causation of pulmonary tuberculosis, and tuberculosis in general, is somewhat different from that which is generally taught, and the treatment advised is, therefore, based upon a conception of the essential nature of the disease, differing in some respects from the generally accepted view. In order that you may understand the reasonableness of the treatment, it is necessary for me to say a few words concerning the etiology and clinical pathology of pulmonary tuberculosis. I assume a full knowledge upon your part of the morbid anatomy and histology so far as determined. Now do not understand me as claiming that the treatment to which these preliminaries are to lead up is novel or a special discovery of my own; there may be some points of detail founded upon personal experience, but the only personal element I wish to impress upon you is a point of view which will necessarily influence your estimate of the relative importance of the different therapeutic measures.

We all know, since Koch has shown us, that in tuberculous lesions there is usually found a certain micro-organism which is called the *tubercle-bacillus*; and we all know that injections of the sputa of tuberculous subjects, or of cultures of the tubercle-bacillus, or of toxic materials containing either bacilli or their products, or products of the organism infected with bacilli, will set up a disease having lesions resembling those of human tuberculosis, in susceptible animals—in rabbits and guinea-pigs particularly. But we also know that injections of any of these substances will not ordinarily set up the disease in certain other animals—in horses or donkeys, for example, or in dogs, or in goats. Evidently, then, the poison administered to the one group of animals being the same as that administered to the other group, while the result is different, there must be some difference in the two sets of animals which causes this difference in result. The only possible conclusion, therefore, is that the tubercle-bacillus and its



products are not alone sufficient to cause the disease, but must act in conjunction with some peculiarity or defect in the constitution of the infected animals; because if the bacilli and their products were of themselves a sufficient cause of tuberculosis they would cause it in goats and dogs and horses just as readily as they cause it in guinea-pigs and rabbits and in certain kinds of cattle.

Extending our observations to man, we find upon clinical evidence that men can also be divided into two groups; that certain individuals cannot be infected with tuberculosis, and that other individuals can readily be so infected. Shall we then say that some persons are "susceptible" to the disease and others "insusceptible" or "resistant," and rest satisfied with that—imagining, perhaps, as is too often the case in medical problems, that by simply re-stating a fact in somewhat technical phraseology we have explained it.

Suppose that it had been determined, in this city, for example, that certain structures were more liable than others to be burned down—that if the torch were applied to certain houses they would quickly be in a blaze, but that certain other houses could not be set on fire, what would be the most rational method of preventing conflagrations? Would it not be to try to make all the houses alike "insusceptible" to fire, and before that was possible would it not be necessary to definitely find out in what the difference consisted that made the one house "resist" and the other "yield?" And the same principle must guide our steps in seeking the rational treatment of tuberculosis. We must not give time to the study of the bacillus alone—we must study it, of course, and find out all there is to find out about it—but we must study more particularly the human constitution and determine definitely why it is that some persons are resistant and others non-resistant to the tuberculating process.

And this is the point wherein my teaching departs from the accepted doctrine. The existence and, at last, the importance, of "susceptibility" is acknowledged by all. A favorite comparison is that of "seed and soil." But I wish to go much farther than this. I hold that normally the human body is never a "favorable soil" for the "seed" of tuberculosis. To be such it must first become abnormal—that is, diseased. In other words, the condition that renders human beings liable to the invasion of tubercle-bacilli, and to the morbid processes thus set up, is the preëxistence of disease; and this disease is something definite, something to be sought, located, described, treated. It is the fundamental pathologic process concerned in the morbid complexus we call tuberculosis; the invasion of the bacillus and its results are epiphenomena. These epiphenomena involve dangers and require treatment on their own account—we must try to put out the fire if possible, but more important is the study and treatment of the condition of primary disease, without which the bacillus might come and go harmlessly. We must try to make our patients *fire-proof*, and they need not fear the incendiary.

But first let us take a step further toward the substitution of a definite conception for the vague term "susceptibility." In guinea-pigs and rabbits lesions histologically indistinguishable from those produced by living bacilli can be produced by dead bacilli, by powdered

glass, and other non-living irritants. Similar lesions similarly caused have occasionally been seen in man. Cornil and Toupet report a case due to irritation by a fragment of oyster-shell. These lesions are called *pseudo-tubercles* nowadays, because they do not contain living bacilli—a remarkable illustration of the fallacy called in logical terminology *petitio principii*, "begging the question" or "arguing in a circle."

In pseudo-tuberculosis we have the histologic lesion, but not the toxemic symptoms of bacillary tuberculosis. But certain forms called "zoöglear tuberculosis" present both lesions and toxemia, although a different microbe from Koch's bacillus is found—probably several different microbes.

The evident conclusion is that long ago formulated by Formad: "a definite soil, an indefinite irritant," for the lesions, and for the toxemia different living organisms and chemic poisons resulting from their development and the reaction of the tissues.

In most cases, I grant, the irritant producing the lesions, and the organism producing some of the toxemia is Koch's bacillus; but even so, it is by no means certain as yet that some of the most destructive lesions and most of the toxemic phenomena are not due to the action of other associated organisms or of poisons produced by them.

Be that as it may, the lesions wherein we can best discover the nature of the "definite soil," in which alone tuberculous lesions can develop, are the pseudo-tubercles, because here the problem is purely histologic and uncomplicated by intoxication and its results. While the morphologic and chemic peculiarities that must exist have not yet been satisfactorily or completely elucidated, it seems to me that a certain broad generalization is fully warranted: that the production of the histologic tubercle is an evidence that *the tuberculated tissue lacks reparative force*. With dead bacilli and mechanical irritants the lesions are strictly localized to the points irritated. With living bacilli they are more or less diffused, because the irritant is reproduced within the body and carried to new areas. Yet in cases killed by "Koch's remedy" recent lesions without bacilli have been found, apparently indicating that under the peculiarly depressing effect of the tuberculous intoxication, tubercles may form even without local irritation.

In normal tissue partial breaking down or complete death of cells through exercise of function, through exhaustion of vitality, and probably through injury, is constantly taking place. As you have seen in non-infected wounds, repair takes place by the production of new tissue-elements or cells, which either produce a new tissue like the original or a fibrous scar-tissue.

In subjects who become tuberculous the vital energy resident in the cells or tissues is not sufficient to produce either new tissue like the original or durable scar-tissue; but when irritated, as by tubercle-bacilli or their toxin, or by powdered glass, etc., or broken down by function or by disease of various kinds, the tissues in an ineffectual attempt at repair produce degraded cells incapable of organization—in other words, tubercle-cells. Tubercle-cells perish from lack of nutriment or under bacterial attack, and ulceration or caseous degeneration takes place. In some subjects, to whom we will recur in a moment, the surrounding tissues react to form a scar, or

calcareous deposit takes place, and healing is thus brought about. The therapist's object is to put all his patients into such a condition that this may be the outcome. To learn how to do it a study of the different degrees of "susceptibility" is necessary.

First, in studying the group of susceptible persons, or as I prefer to call them, using a term introduced by Jaccoud, *hypotrophic* subjects (*hypo*, not *hyper*), we find them sharply divided into two classes—those in whom the condition is congenital, and those in whom it has been acquired after birth. Therapeutics can do most with the latter class, but much even with the former class. The congenitally hypotrophic may further be divided into those in whose families tuberculosis is hereditary, and those in whom other conditions than parental tuberculosis have given rise to hypotrophy in the offspring. For instance, carcinoma, syphilis, diabetes, alcoholism, and other conditions which lower the parents' vital energy make the children prone to tuberculosis. Among those who are not congenitally hypotrophic, the condition may be induced by privation, by excess, by depression. Thus it may be due to a life of poverty, or of debauchery, or of overwork; to the effects of influenza, of typhoid fever, of measles, of diabetes; to residence in damp places, especially if light and air are excluded; to severe exposure; to the exhaustion of prolonged lactation, or of too many or too frequent pregnancies. The rigorous practices and depressing doctrines of certain religious sects and orders notoriously render their members liable to consumption, as may mental or emotional shock or profound grief, as in disappointed love or defeated ambition; and one of the most potent causes is confinement, as in prison, or in certain occupations which must be carried on indoors, in crowded, ill-ventilated, and over-heated rooms.

Thus carnivorous animals, which in the wild state are probably not liable to tuberculosis, almost invariably die of this disease after a certain period of captivity; and, on the other hand, even animals so susceptible as rabbits may escape all evil consequences from inoculation with virulent cultures of tubercle-bacilli if, after infection, they are allowed liberty in the open air and sunlight.

Graves, one of the greatest of clinicians, said: "It is important to know how to make a man phthisical, as by pursuing an opposite course we may prevent it." The deduction from what I have said is so obvious that I need not take time now to elaborate it. One point only let me make in passing: If you want to keep healthy lungs you must give them facility for proper development in the open air; if you want to make tuberculous lungs healthy once more, you must exercise them in the open air; or, if they cannot properly exercise unaided, you must resort to artificial means, such as the inhalation of compressed air.

To resume our study of that particular group of persons who are susceptible to the action of the exciting causes of tuberculosis, we find certain differences in their susceptibility. In some the fire, once started, burns rapidly, and cannot be estopped until the structure is destroyed; in others it can be checked after it has burned half-way, or less, but remains smouldering indefinitely, ready to flame up again; in others, it can be quickly checked, and finally brought to an end.

We find, for example, a patient like Henry D., in whom there is a certain tendency to a limitation of the morbid processes, purely by the natural powers of reaction. Pathologic processes, like all others, can be limited in two relations—duration and extension, that is, in time and in space. We have extension in space and duration in time. In our tuberculous cases we must study the natural tendency of the human organism to limit the disease, and whether these natural powers limit the extension of the lesions in space, or the duration of the process in time, or both. Let us take, for example, the case of a normal or nearly normal man—one of the "insusceptible" group—who is performing the autopsy of a tuberculous subject, and is inoculated with the products of the disease. What happens? There may be no result, or there may be developed upon a wounded finger the "anatomist's tubercle." You have seen the growth—it resembles a wart. It is the result of a tuberculous process. But, although there are bacilli present, and histologically the characteristic cells of tuberculosis, the morbid product does not extend in space and the morbid process does not endure in time. Even if left to itself, the growth will in most cases disappear, and it always disappears very quickly if treated with lactic acid—a very simple therapeutic measure. This is an illustration of the highest tendency to limitation both in space and in time. Next in order, confining ourselves now to pulmonary lesions for the sake of simplicity, is the condition termed by Sir Andrew Clark "fibroid phthisis," or later, "fibroid tuberculosis." Histologically, tubercles consist of aggregations of degraded tissue-elements incapable of progressing to the formation of tissue like that from which they took origin, or even of durable scar-tissue. They are devoid of vascularity, and break down from want of nutriment, or under bacterial attack. Healing can take place only through the formation of scar-tissue, fibrosis, fibroid proliferation, by surrounding tissue. In fibroid phthisis cicatrization keeps pace with ulceration, the lesions are long confined in space, and the patient retains a fair degree of general health. Such cases are frequently mistaken for asthma. The process, however, is not limited in time, and may last for twenty or even forty years, the patient dying sometimes of intercurrent disease. In some instances the extension is at times, or toward the end, rapid, and the patient does not survive so long.

In Henry D., and in the man beside him, we have illustrations of a third degree of tendency to limitation in space, without much limitation in time. The process in these patients is confined to a limited area, principally at the apices of the lungs, and extends very slowly, but yet endures. It has endured, under our own observation, for five years in the one case, for three years in the other, notwithstanding the fact that the patients' resistance has been increased by treatment. In Henry's case there are two large cavities, one in each lung, in which from time to time we detect signs of activity, which, however, subside under treatment, and even without treatment. In another group of patients we will have an exactly opposite tendency. General reaction is good when aroused, but there is little local resistance. The lesions are but feebly limited in space, but there is a strong tendency to limitation of the morbid process in time; that

is to say, so long as it lasts it spreads more or less rapidly, and may involve a considerable extent of pulmonary tissue, yet it comes to an end in a comparatively short time. An illustration is shown by the patient G., in whom at present all active manifestations of the disease have ceased, but who has in a few months lost so much pulmonary tissue that a cavity in the middle of the right lung is easily demonstrated. The question, in a patient of this kind, is: Will the time-limitation come into play before the destruction of tissue has gone too far? Nature has been bountiful to us in the matter of lungs and other organs, and a total loss of half the normal amount of pulmonary structure is not incompatible with good health, as is shown by a remarkable case of Tillmann's, in which disease and surgery had practically removed the whole of one lung, the other lung remaining healthy.

And then we have, unfortunately, another group of patients, those in whom there is no tendency to limitation either in space or time, and they form the type of what is termed "galloping consumption." The morbid process extends rapidly, and is not limited, except by the death of the patient. I have never seen cases of that kind recover. Of the other classes, I have in many, many cases seen the recovery prolonged for a sufficient number of years to enable one to say positively—yes, this patient has recovered.

In some patients who exhibit a tendency to limitation of the process in time, or limitation of extent of the lesions in space, there happens a series of phenomena which it is very important for us to understand. The processes apparently come to an end; the patient is to all appearances well; upon physical examination we elicit not the signs of an active process, but merely the signs of the result of previous destruction. That state of affairs continues a number of months, or a number of years, and suddenly, without any well-recognized cause, or perhaps through the intervention of some exposure, or of something which reduces the vitality of the patient, such as mental anxiety or bad nutrition, everything starts up again. The patient gets into a very alarming condition, and the family expects his immediate dissolution. Then, just as the process stopped the first time, it stops again before very much damage has been done. We may thus have a case which goes on for almost an indefinite number of years with alternating periods of apparent good health and of acute disease. I have in mind now a case which has been under my observation for many years, in which there have been three such attacks, and yet the patient (from a letter which I received about two weeks ago) is now in such good condition that she weighs about 175 pounds; has no cough, no fever, and is, to all intents and purposes, well. That woman has been in an alarming condition, under my own observation, at least three times. Here is one illustration of what I mean by "preaching the gospel of hope." Never give up the ship in any case of pulmonary tuberculosis! You do not know until the final issue whether or not it is a case susceptible of limitation; and the harder you try, the greater will be the number of cases in which you will bring about the limitation in space, sufficient to save life for the time, even if the process remains latent, to break out again. Even if you strive hard in ten cases, and only one of them shows a good result, it is worth

wasting your work on the other nine to save that one. But the proportion of successful results possible is much larger than one in ten. Under favorable circumstances—if we exclude galloping consumption and acute miliary tuberculosis—I should consider 50 per cent. of recoveries much under the mark. Further than this, even when recovery is impossible, life can be prolonged and suffering can be relieved.

The degree of success achieved will depend upon the hopefulness with which the treatment is undertaken and the persistence with which it is carried out, notwithstanding the supervention at times of alarming symptoms. Now, what is the proper line of treatment? I have already said that it is devoted almost entirely to increasing the resisting power of the patient. Let the bacillus alone and strengthen the patient, and if you strengthen the patient to the proper point, his own vital forces will be sufficient to take care of the bacillus. I think the French records show that in at least 50 per cent. of the cases submitted to autopsy in which there has been no other evidence of tuberculosis, clinically or pathologically, the bronchial glands are found to contain tubercle-bacilli. What does that mean? It means that all of us are practically attacked by tubercle-bacilli all the time, and that those of us who are in a normal condition possess the requisite amount of vital energy to enable the bronchial and other glands to closely imprison and finally destroy the invaders, and no harm is done. Therefore, let me once more repeat, for this is the lesson I want to teach, that the aim of treatment must be to make other people as strong as those who resist naturally. You remember what Oliver Wendell Holmes says in *The One-Horse Shay*, and it is good policy even in medicine:

" . . . It's mighty plain  
The weakest place must stan' the strain;  
An' the way to fix it, ez I maintain,  
Is only jest to make that place as strong as the rest."

You remember that the one-horse shay ran for one hundred years without breaking down and then crumbled into powder. What the deacon did as a carriage-builder we must try to do as physicians—*make the weakest man as strong as the rest*. In the case of patients whom we know to be by heredity liable to tuberculosis, we must early institute preventive treatment. This has two branches: First, the avoidance of infection. Of this I will not now speak, except to emphasize the necessity for teaching your patients not to spit around indiscriminately, but to take such care of the sputa and other discharges that they may not become dried, ground into dust, and being wafted about by the air become a source of danger to others. Avoidance of infection is not always within our control, therefore the other branch of preventive treatment is even more important. This consists of invigoration, or in other words the reinforcement of vital energy.

The methods by which we may bring about this reinforcement of the vital energy of the patient are fortunately quite simple; the details are very many. To lecture upon the details would take several hours, but the principles are few, and if you once have a grasp of the principles you can make the detailed applications of them from your own common sense. The principles are the same, whether applied in prophylaxis (in the



endeavor to prevent disease) or in therapeutics (in treatment having for its object the facilitation of recovery). We must try to control the daily life of the patient—control it, so far as may be possible. First, in relation to his place of residence. That includes his exposure to light and air. The subject of climate cannot be treated in the few words now at my disposal. But this I will say: while proper climatic treatment can do much good, improper climatic treatment does much harm. One climate is not suitable for all cases, just as one coat will not fit everybody. As a rule patients with a robust tendency do better in cold and elevated regions, while those who are in need of soothing and protective measures, who easily take "cold" and have a tendency to fever or nervousness, are better off in warm, and if possible, comparatively dry places. Dryness is a great desideratum always, but it must not be excessive, as it may provoke hemorrhage. The open sea is beneficial in nearly all cases, but the seashore is to be avoided whenever active destruction of tissue is in progress. Many patients cannot avail themselves of change of climate. Such patients must be gradually encouraged to be much out of doors in all weather, proper means of fortifying them against violent changes being resorted to. Secondly, we must control the patient's clothing. We must see that he wears such clothing, light and porous, and not constricting the body at any point, as will best protect him from vicissitudes of weather. Woollen or silk underwear is preferable, as it keeps the skin, as regards moisture and the body-temperature, as nearly as possible in the same condition in different circumstances of weather. Women must be told to give up corsets, tight-waisted bodices, and the like, and all must avoid overheating scarfs, boas, and the like around the neck. Waterproof coats and overgarments are objectionable. Thirdly, we must control the patient's food; administering such aliment as will yield the greatest amount of energy with the least expenditure of vital force in its digestion and assimilation. Nitrogenous and fatty foods, predigested if necessary, and a moderate amount of greens, best fulfil the requirements. In most cases overfeeding, six meals a day, or feeding with the stomach-tube twice a day, will be useful. Fourthly, we must control the patient's activities. We must regulate his exercise and his rest. One authority will insist upon rest in the treatment of pulmonary tuberculosis, another with equal propriety urges the importance of proper exercise. They are both right, both wrong. Whoever urges rest without exercise looks only at one side of the shield; whoever urges exercise without rest looks only at the other side of the shield. You must regulate both—the patient's rest and the patient's exercise. When there is a continuous tendency to fever, the temperature reaching or exceeding 100° F., put the patient to bed and use ice to the chest, over the heart, until the fever is controlled. Under all other circumstances see that the patient spends as much time as possible out of doors, walking, riding, cycling, driving, but never to the point of fatigue. In other words, exercise graduated according to the patient's ability is what is needed. We must likewise take into consideration the patient's vocation and his avocations—that is, his ordinary pursuits, the work he does for his livelihood, and those pursuits which he takes up for recreation and

amusement. These must be so arranged, if possible, as to be a part of the treatment. A business that keeps the patient out of doors is the one to be preferably followed; and a most important therapeutic factor in putting the patient into that condition where he can recover, is to afford him cheerful and innocent amusement. Fifthly, we must teach our patients to use their lungs properly, and we must dilate the unused or little used portions. This is accomplished by certain gymnastic measures and by the inhalation of air at modified pressures. Patients can be overfed while inhaling compressed air, who could not be overfed otherwise. Sixthly, we must insist upon the free use of water internally and externally. The proper, judicious use of cold water, by douching, sponging, bathing, and the like is one of the best measures we have for stimulation of nutrition and for education of the vessels to resist changes of weather. The drinking of hot water before meals keeps the digestive tract clean and facilitates secretion and excretion generally. This is especially necessary when large quantities of meat are eaten. Seventhly, we must pay careful attention to the patient's secretions and excretions, endeavoring by hygienic or medicinal means, as necessary, to keep them quantitatively and qualitatively normal. Finally, and least important—and yet of very great value—is the administration of drugs by the mouth, by inhalation, by the skin, or in any other way. I do not underrate drugs; I know we get much good from the use of drugs in the treatment of pulmonary tuberculosis, and I would not like to have to give up such agents as arsenic, strychnin, iodine in its various forms, alcohol, turpentine, hydrogen dioxid, and creosote. But if we should be compelled to choose between hygienic measures and drugs, I would unhesitatingly prefer the former. I believe that a greater proportion of cases would recover under the proper use of rest, exercise, air, sunlight, food, and water, than under the most approved medication with the neglect of the other means. And that is, I think, the reason why some physicians have taken too hopeless a view of the possibilities of therapeutics in the treatment of this disease; they have concentrated their attention upon drugs; they have looked for some specific which would work a miracle and change the constitution of a patient in an hour, or a day, or a year. It is an impossibility. But if, instead of simply writing a prescription for cod-liver oil or hypophosphites, which were the fads at one time, or for creosote, which is now the fad—all good fads in their way—if, instead of saying, "take this three times a day," the physician will talk with the patient for half an hour as to just how he is to live, just what he is to expect in the natural process of the disease, how he is to guard against different accidents which occur in the course of the disease; and then, not only that, if every time the patient comes we question him closely: "What are you eating, drinking, doing in the way of rest, exercise, amusement, and occupation?" and insist that our directions shall be carried out literally and exactly, we will accomplish results which cannot be accomplished in any other way. And therefore I desire you to make every case of pulmonary tuberculosis that comes under your care an individual study, giving such drugs as may be indicated, and concerning some of these we will have a talk hereafter, but above all regu-

lating the daily life of the patient in all the different relations of which I have spoken, in accordance with the different conditions which each particular case may exhibit.

## CLINICAL MEMORANDUM.

### PRACTICAL APPLICATIONS OF POTASSIUM PERMANGANATE AS AN ANTIDOTE TO LAUDANUM.

BY EDWARD ROLLIN GREGG, M.D.,  
AND

G. B. MORELAND, M.D.,  
OF PITTSBURG, PA.

On the evening of March 13, 1894, the patrol-wagon brought to the Homeopathic Hospital of Pittsburg, at 10 o'clock, George D., in the third stage of opium-poisoning. He had taken two and a half ounces of laudanum at about 7.30 o'clock that evening. When discovered, in the stables of the Eureka Ice Co., his breathing was stertorous and he was entirely unconscious. A doctor was called, who treated the patient until nearly 10 o'clock, and then sent him to the hospital as a last resort.

Upon admittance the man was in the following condition: The face was livid, the lips blue, the respirations shallow and four per minute, the pupils smaller than a pin-head and inactive, the corneæ insensitive to touch, the pulse weak, the bladder distended. The patient was in a state of most profound coma. Pressure over the supra-orbital nerves failed to elicit any response.

Artificial respiration was at once practised and the patient was catheterized.

From the apparent hopelessness of the case it was decided to try the newly suggested and experimental antidote—potassium permanganate. A solution of this salt was hurriedly made (which was afterward computed to be one-half the strength of a saturated solution), and two drams injected hypodermatically into the left arm at ten minutes after 10 o'clock. Artificial respiration was continued, the stomach was washed out with water, then with coffee, and one pint of coffee was allowed to remain in the stomach.

The man's condition remaining unchanged, he was given  $\frac{1}{100}$  of a grain of atropin hypodermatically at 10.45 P.M.; and  $\frac{1}{120}$  of a grain of atropin in the same manner at about 11 o'clock, but with no effect, not even upon the pupils. Ice-water was thrown over him and he was slapped with towels, but all in vain.

At 11.45 P.M. the man was barely alive; he was then given another two drams of the same permanganate solution hypodermatically. This injection seemed to increase the depth of the respirations very slightly, but otherwise there was no change. At 12.15 A.M., March 14th, he was given still another hypodermatic injection of two drams of the permanganate. Almost immediately there was noticed a perceptible increase in the depth and number of respirations; the face became less livid, and the patient's condition seemed slightly changed for the better. At 12.30 A.M. a slight movement of the fingers was noticed, and the man was again given a hypodermatic injection of two drams of the same solution of permanganate. The effect of this was noticed almost at once. The patient put

his hand to his mouth, his respirations became still deeper, his face and lips became brighter, and a sigh escaped him. At 12.45 A.M. he was given still another hypodermatic injection of permanganate of the same strength and amount. Immediately he put his hand to his mouth, moved his legs, opened his eyes for a moment when loudly spoken to, and in response to a question tried to answer, but could not articulate. At 1 A.M. another injection of the same strength and amount as before was given him. This increased the depth and number of the respirations still more and his corneæ became sensitive to touch for the first time.

At 1.15 A.M. the man's respirations were full and 10 to the minute. He opened his eyes and answered "What," when called by name, but relapsed into a lethargic state, from which he could be aroused by speaking loudly to him. The corneæ were now very sensitive to touch. He was again catheterized and twelve ounces of urine withdrawn.

At 1.50 A.M., after a brisk slapping he recognized his employer, who was present.

At 2.15 A.M., the respirations were 10 per minute and full, the pulse was 104, and the temperature 96°. The patient now answered questions, but wanted to sleep. He was now pronounced out of danger, was left in the care of a nurse, and Dr. Moreland and I retired.

At 4 A.M. I was called and found the patient rather stupid, but otherwise in good condition. He talked intelligently, although his pupils were still contracted and inactive. I now ordered him to be walked until I should see him again, which I did at about 8.30 A.M. on March 14th. I found him at this time perfectly sensible, though shaky and trembling. He informed me with a smile that he was "dead tired" and would like to go to bed. I allowed him to sit down, with someone to watch him, but he did not go to sleep.

About 10 A.M. he had nausea and vomited. At noon he was still shaky, his tongue dry, and he said that he had no appetite. However he drank a bowl of milk for his dinner. He complained all the afternoon of being tired, but he was not allowed to go to bed. At 5 P.M. his respirations were 18, the pulse 82, and the temperature 100°. For supper he ate some bread and drank some tea. In the evening I found him as sensible and talking as intelligently as could be desired. His left arm was now quite painful. He had been given hypodermatic injections in each arm, those in the left rather deeply. He was allowed to retire early, but the pain in his left arm kept him awake the greater part of the night.

The next morning (March 15th) his condition was improved, though his tongue was still dry. He was bright and clear-minded. Not until toward evening did his pupils return to their normal size and react to light. His arm was much swollen, very painful, and quite red; the redness extending from the shoulder to the elbow and increasing in intensity. At 5 P.M. his respirations were 24, pulse 112, and temperature 102.4°.

From this time until March 18th the man's condition remained about the same. An incision about two inches long was made in the left arm followed by a gush of about twelve ounces of pus, which was thin, brown, and malodorous. The night of March 18th the man was slightly delirious, but the temperature was only 100.4°. The night of March 19th there was a high delirium all

night, though unaccompanied by violence. At 4 A.M. of the 20th, the thermometer registered 99°, the respirations were 20, and the pulse 80, continuing the same during the day. The swelling and redness were disappearing rapidly from the arm. The delirium continued during the next two days unabated. On March 22d, fluctuation was detected in the right arm at the seat of the injections. Incision gave vent to about two ounces of odorless pus. The temperature now became about normal, and the arms better in appearance, with lessening of the delirium. From this time the man's condition steadily improved in all ways, and he was discharged cured on March 31st.

I cannot readily account for the delirium in this case. It did not appear until *after* the pus had been evacuated from the left arm, and continued for several days, while the temperature was not over 101°, and sometimes normal. After the pus in the right arm had been freed the delirium subsided, and yet I cannot think that it was due to this retained pus, for there was but two ounces, and the inflammation in that arm had been very mild, with very little pain or inconvenience. I have not been able to decide satisfactorily whether the delirium was due to the permanganate, to the after-effects of the opium, or to the previous drinking-habit of the patient. Certain it is, that in the following case of Dr. King's, there was no delirium following the administration of permanganate, although there was an inflamed arm, but no abscess.

In this case the permanganate received a crucial test (as the man could have been in no worse a condition and living), and it proved itself worthy.

On March 14th, the day following the admission of this patient, a patient suffering from laudanum-poisoning was taken to the Allegheny General Hospital, and permanganate of potassium was administered. The patient returned to consciousness soon after the administration of the permanganate. Unfortunately, the amount of laudanum given could not be ascertained, so that in this case the quick return to consciousness cannot positively be credited to the permanganate; that there was a marked improvement after the injection of the permanganate cannot be denied.

A few days later, Dr. W. D. King, of this city, was called to see a case of accidental poisoning in a woman who had taken one ounce of laudanum. When Dr. King arrived the woman was in a comatose condition. He immediately injected half a dram of a saturated solution of permanganate of potassium hypodermatically, and in one hour left the house pronouncing the patient out of danger. Dr. King does not claim that *only* the permanganate would have saved her life, but he does claim that it saved him the trouble and fatigue of walking his patient for several hours. These two cases I took the trouble to investigate, and give the facts as they were told to me.

These cases seem almost sufficient to prove that permanganate of potassium is the quickest-acting, if not the best, antidote to opium and its derivatives.

*Dr. Juhel-Renoy*, a promising Parisian physician, died recently, at the age of thirty-nine years, of typhoid fever. He made numerous valuable contributions to medical literature. His most recent work was, perhaps, his advocacy of the cold-bath treatment of typhoid fever.

## HOSPITAL NOTE.

### EXTRAVASATION OF URINE; EXTERNAL PERINEAL URETHROTOMY WITHOUT A GUIDE; DEATH FROM UREMIA.

*Surgical Wards, Philadelphia Hospital.*

SERVICE OF ERNEST LAPLACE, M.D.

[Reported by ALVAH M. DAVIS, M.D., *Resident Physician.*]

L. B., aged thirty-one, a bookbinder, and a native of Philadelphia, with a negative family history, had enjoyed perfect health until one year ago, when he contracted an acute gonorrhea, which lasted three weeks and left him with a slight discharge, which persisted up to the time of entering the hospital.

On admission, some dysuria with *ardor urinæ* and pain over the region of the bladder was complained of. The temperature was 99.4°, the pulse 68, the respirations 24. Physical examination showed the bladder to be somewhat distended and tender on pressure, and on squeezing the urethra a few drops of thin, gleety discharge exuded. The epididymis, testicles, and scrotum were apparently normal. Fully a quart of urine, very turbid and filled with flocculi, was withdrawn by catheterization. Examination showed the fluid to be of acid reaction, to have a sp. gr. of 1025, the nitric-acid contact-test showing a faint ring of urates; no albumin or sugar was present, but there was considerable mucus. Microscopically, bladder-cells, shreds of mucus, uric-acid crystals, and crystals of oxalate of lime were seen. On exploring the urethra, a stricture of large caliber was detected just anterior to the bulbo-membranous junction, admitting a No. 20 sound. After dilatation of the stricture the patient was able to void urine more freely, but complained of severe pains in the lumbar and suprapubic regions, with extreme prostration. The temperature became remittent or hectic in type, ranging from 102° to 103° evenings, to from 99° to 100° mornings. Stimulants and diuretics were given, and the bladder washed out daily with a quart of equal parts of distilled water and a hot, saturated solution of boric acid. Four days after admission the skin at the junction of the penis with the scrotum became inflamed, swollen, and tender to the touch, and on careful palpation an indurated area about the size of a peach-kernel could be detected, extending on either side of the median line. At this time considerable pain was experienced during the act of micturition, the patient being able to expel a few drops of urine only with violent straining efforts. On exploring the urethra it was found impossible to pass even a filiform bougie, enough urine, however, being voided to keep the bladder from becoming over-distended. Opium by the rectum, and hot compresses to the perineum were employed without affording much relief. The area of induration increased rapidly in extent, the skin of the prepuce and scrotal tissues becoming markedly edematous, and, forty-eight hours after the appearance of the extravasation, operation was considered advisable. The seat of the operation having been properly prepared and the patient anesthetized, an incision, about three inches in length, was made in the median line of the perineum, entering the urethra at its membranous portion.

About six ounces of acrid urine, mixed with pus,



escaped. A sound was then introduced into the bladder, and the incision continued upward in the urethra, cutting through the stricture. At this point some pus escaped, demonstrating the existence of a peri-urethral abscess. A staff being next inserted, the portion of the urethra containing the stricture and fistula was completely obliterated. During the operation considerable venous oozing occurred, but very little arterial hemorrhage. The wound, after being thoroughly irrigated, was packed with iodoform-gauze and the dressing held in place by means of a T-bandage. The temperature immediately after the operation was 102°, the pulse 112, the respirations 28. The heart-sounds were feeble, the muscular element of the first sound being especially weak. Urine trickled through the wound, and the edema of the prepuce and scrotum disappeared to some extent. Unfortunately, because of the constant escape of urine, the quantity secreted in twenty-four hours could not be measured, nor could it be examined subsequently for the presence of albumin. Internally, sulphate of strychnin, tincture of ferric chlorid, and whiskey were given, together with diuretics, but the patient's condition did not improve. A low form of delirium developed, accompanied by carphology and *subsultus tendinum*, the temperature (after the shock of the operation) remaining continuously high, with slight remissions. On the third day following the operation an attempt was made to repack the wound, when considerable venous hemorrhage occurred, being controlled with difficulty by constant digital pressure. The surrounding tissues looked flabby and indolent, showing no signs of granulation. Twenty-four hours later urine ceased to flow through the wound and another attempt was made to repack it, when venous hemorrhage again occurred and was again controlled with difficulty. On the fourth day after the operation the respiration became of the Cheyne-Stokes type, the delirium giving place to stupor and coma, the pupils became dilated, the pulse feeble and fluttering, the temperature subnormal, and death quickly followed.

The clinical diagnosis of the cause of death was uremia and extravasation of urine.

At the autopsy the following interesting conditions were noted: The weight of the left kidney was 5.5 ounces. The capsule was adherent in places, but otherwise stripped off readily and to an abnormal degree. The cortex was somewhat swollen and studded with minute, white, slightly elevated points, which extended downward into the pyramids. At one point in the cortex there existed a crater-like cicatrix, possibly gummatous. The left suprarenal capsule showed marked pigmentation; the right was normal. The pelvis of the right kidney was distended with a cloudy, serous fluid with a slightly ammoniacal odor. The cortex was striated and fatty, the same elevations being noted as were seen in the other kidney. The organ weighed 6 ounces. The tissues surrounding the wound were in a sloughing condition: namely, the scrotum and its contents, and the membranous and bulbous portions of the urethra. The heart weighed 12.5 ounces, and showed some hypertrophy of the left ventricular wall, but it was otherwise normal.

The other organs were in apparently good condition.

The pathologic diagnosis was: Large white kidney; acute hydronephrosis on one side; sloughing of the external genitals and perineal contents.

## MEDICAL PROGRESS.

*Esophagotomy and Gastrotomy for the Removal of a Denture from the Esophagus.*—WALLACE (*Lancet*, No. 3862, p. 734) has reported the case of a woman, twenty-eight years old, who during sleep accidentally swallowed a denture with five teeth attached, the presence of which in the esophagus was with considerable difficulty detected by means of a coin-catcher. Attempts at its removal without undue force, however, failed, and its position having been located at about midway between the manubrium sterni and the stomach, esophagotomy was performed as low down as possible. Even now the plate could not readily be removed, and, accordingly, gastrotomy was undertaken. After a little difficulty the esophageal opening into the stomach was found and the denture was then easily extricated with the aid of a pair of dressing-forceps. The mucous membrane of the stomach was brought together by a continuous suture and the serous coat by the Lembert suture. The wound in the abdominal wall was closed in the usual way. The esophageal wound was closed with fine catgut and the superficial parts with interrupted sutures, a small piece of iodoform-gauze being introduced to facilitate drainage. Dressings were then applied and the patient was put to bed. For forty-four hours after the operation nothing was given by the mouth, but nutrient enemata were given every four hours. For five days the enemata were retained and gave rise to no irritation. Morphin suppositories were introduced thrice daily. On the sixth day a slight diarrhea set in and continued for a week, although the enemata were withheld. Forty-four hours after the operation the strip of gauze was removed from the esophagotomy-wound, which was healthy in appearance and presented no evidence of saliva having escaped into it. A smaller, fresh piece of gauze was introduced, the wound having been powdered with iodoform. Two ounces of milk and barley-water were given by the mouth, but none escaped into the esophagotomy-wound. Thereafter, milk, cream, and barley-water were administered every four hours. On the fifth day, however, it was noticed that a little milk escaped into the esophagotomy-wound. It was, therefore, concluded to feed the patient through a tube passed through the mouth into the stomach. For a time the woman did badly, losing ground, for a brief period being actually maniacal, and her management was attended with a good deal of difficulty. By careful attention to the esophagotomy-wound cicatrization was brought about after the lapse of a month, and a few days later the woman was dismissed, cured. The report is concluded with the following propositions: 1. It is extremely dangerous to leave a foreign body in the esophagus, as disastrous results almost invariably ensue. 2. Such disastrous results may not occur for some time after impaction. 3. If attempts at removal of the foreign body with instruments fail, one should at once proceed to operative interference. 4. The operation of esophagotomy is not, *per se*, dangerous. 5. If the body be impacted low down, near the cardiac opening of the esophagus, gastrotomy is preferable to esophagotomy when the body is irregular. 6. By one or the other of these operations all parts of the esophagus are within reach. 7. The danger of opera-

tive procedure is mainly due to the difficulty of alimentation. 8. The superficial parts in the esophagotomy-wound should be left freely open, so as to heal by granulation.

**Merycism.**—From long and careful observation of a case of merycism in a male and from a study of the literature of the subject, LEMOINE and LINOSSIER (*Revue de Médecine*, 1894, No. 3, p. 200) conclude that this condition should be considered as presenting two varieties—a simple form and a pathologic form; while the latter includes three sub-varieties—a neuropathic form, with or without dyspepsia, a form of dyspeptic origin and directly related to the dyspepsia, and a form of dyspeptic origin but not disappearing with the relief of the dyspepsia. The condition must in its intimate nature be considered a gastric neurosis. The act of regurgitation is a reflex phenomenon whose point of departure resides in a congenital or acquired exaggerated sensibility of the mucous membrane of the stomach. The mechanism of the condition corresponds with that described in ruminants, and consists essentially in a spasm of the diaphragm and of the muscles of inspiration, diminishing the intra-thoracic pressure and tending to dilate the esophagus, while the abdominal muscles compress the stomach and cause the expulsion of its contents.

**Appendicular Colic.**—JESSOP (*British Medical Journal*, No. 1734, p. 627) contends that the vermiform appendix is liable to partial occlusion of its canal from various causes, some of which are permanent whilst others are transient. The symptoms by which such incomplete obstruction is to be recognized are those of appendicular colic. In cases of recurring appendicular colic, and especially if there be at the same time an increasing severity, the practice should be to recommend the removal of the appendix. The time is believed to be past when such misleading terms as typhilitis, perityphilitis, cecal and pericecal abscesses should longer be applied to diseases having their origin in the vermiform appendix, as appendicular colic, appendicitis, and appendicular abscess more accurately describe the conditions present.

## THERAPEUTIC NOTES.

**The Treatment of Typhoid Fever by the Ingestion of Large Quantities of Water.**—MAILLART (*Revue de Médecine*, November 10, 1893, March 10, 1894) maintains that the treatment of typhoid fever by the ingestion of large quantities of water is worthy of recognition as a useful therapeutic measure. In order that the treatment shall be efficacious the patient should receive from five to six quarts of water daily during the febrile period. There is no contra-indication to this mode of treatment; weakness of the heart constitutes an indication rather than the reverse. The results obtained by this mode of treatment consist in a progressive subsidence of the febrile process, a disappearance of the dryness of the tongue, and a marked sedative influence upon the nervous, circulatory, and renal phenomena. These results are to be ascribed to the oxidation, the solution, and the elimination of the toxins produced in the progress of the disease and also of the dejecta. The oxidation is indicated by an

increased production of urea, and its elimination by the skin and the kidneys in the profuse perspiration and abundant diuresis. The diuresis aids in the restoration of the integrity of the kidneys, as shown by the disappearance of the albuminuria. This mode of treatment has no noteworthy influence upon the course, the duration or the evolution of the disease. It is not attended with any unpleasant complications. It has the great merit of facility of application, and the coöperation of the patient is readily obtained.

**Chloroform and Morphin in the Treatment of Chorea.**—CHURTON (*British Medical Journal*, No. 1734, p. 633) has reported the case of a girl, seventeen years old, with a rheumatic history but no existing arthritis, who came under observation in an attack of moderately severe chorea. The movements increased rapidly in violence, unchecked by chloral given frequently in doses of 20 grains. Beginning with gr.  $\frac{1}{4}$  of morphin hypodermically, immediately followed by the inhalation of chloroform for a few minutes, and gradually increasing the dose of morphin to gr.  $\frac{1}{2}$ , always in conjunction with inhalations of chloroform, the movements were kept under control with the greatest ease. The smaller doses produced sleep lasting from one to three hours, the patient waking no better; but after the dose of gr.  $\frac{1}{2}$  she slept almost uninterruptedly for nine hours, and was then so greatly improved that she required no further medication except by the mouth—in fact became an ordinary, mild case, and left the hospital well at the end of about six weeks. It is pointed out that with moderate doses of morphin there is often a delay of many minutes before sleep occurs; the inhalation of chloroform for two or three minutes produces immediate sleep, which is continued by the action of morphin. If necessary the chloroform may precede the injection of morphin.

**The Treatment of Fracture of the Patella by Arthrotomy and Metallic Suture.**—After an experience of five years, covering fourteen successful cases, LINGER (*Journal des Praticiens*, 1894, No. 24, p. 283) recommends the performance of arthrotomy, with suture of the fragments, in the treatment of fracture of the patella. He details the steps of the operation as follows: The knee is made aseptic by means of washing with soap and carbolized water; and is then rubbed with ether. An incision is made in front of the patella, the articular cavity is irrigated with a 2.5 per cent. solution of carbolic acid, and drainage is provided for in the most dependent situation. At least two holes are bored in each fragment, and through these strong silver sutures are passed, and approximation effected by twisting. The periosteum and the fibrous tissues are sutured with silk thread, and the parts are again irrigated with a 2.5 per cent. solution of carbolic acid. The cutaneous wound is sutured with silver wire, and the dressing is completed with iodoform-gauze and absorbent cotton. The extremity is placed in a position of extension, and immobilized in a splint. The treatment occupies four weeks. The drainage-tube may be removed at the end of a week. The union is believed to be osseous. The only contra-indications to this method of treatment are furnished by nephritis, diabetes, obesity, and cachectic conditions.

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SATURDAY, MAY 5, 1894.

## CRANIOTOMY, CESAREAN SECTION, AND SYMPHYSIOTOMY, FAIRLY CONSIDERED AS OBSTETRIC EXPEDIENTS.

1. *Craniotomy* is a very ancient operation, so old that we can form no idea when it began. It appears to have suggested itself to Arabian and Japanese obstetricians many centuries ago, as a means of saving the life of the mother, at the expense of that of the child, an excuse for which lay in the fact that there was no other alternative method of delivery. So the head was opened and emptied, a hook or hooked forceps applied, and the fetus delivered if practicable, but if not, the mother died undelivered.

The first alternative method was that of the Cesarean operation, introduced in 1498, which enabled the obstetrician to deliver the fetus in cases in which the pelvis was too small to admit of its passage after craniotomy. To save the fetus alive at an additional risk to the mother was scarcely ever thought of, and a resort to the method was rarely decided upon until the woman had been long in labor. The operation was soon regarded as almost an equivalent to a doom of death, and the dread of it, begotten of delay, became a most potent factor in rendering its fatality certain. No one ever sewed up the uterus, and its atony, en-

gendered by muscular fatigue, left an open wound for leakage into the abdominal cavity of a fluid that in nearly all cases produced peritonitis and death. This neglect of the uterine wound is to us unaccountable, and especially as we know that the first man who ventured to sew it up was bitterly censured, although the woman recovered.

The second alternative method was the *free* introduction of the use of the obstetric forceps, in 1733. This instrument is an alternative only to a limited degree in saving the fetus; but within this limit it has enabled the obstetrician to save many children that would otherwise have been sacrificed. It was a great improvement upon the Cesarean delivery of the period, as, the element of fear being absent, there was no incentive to delay in resorting to its application.

2. *The Cesarean operation* of to-day has nothing of the original section left in it, except that the two incisions are in the median line. The first vital improvement was the sewing up of the uterine wound; the second was operating early in labor; the third was the use of anesthetics; the fourth was the adoption of antiseptic treatment before and afterward; and the fifth was the multiple suturing of the uterus in two rows, deep and superficial. The end and aim in this operation for the last fifteen years has been to reduce its mortality, and this has been accomplished to a marvellous degree in certain Continental maternities, notably those of Leipzig, Dresden, and Vienna. The possibilities of the operation are best attained in hospital service, and under operators whose repeated sections under favorable circumstances make terminations in death very exceptional. It is not at all difficult to predict before an operation what will in all probability be its result. This has been done repeatedly, and every favorable prognosis was followed by recovery—a result that is largely due to the prior condition of the woman. Cesarean section, considered in itself, may be regarded as an operation having comparatively little danger. Few men believe this, and some think it an extravagant opinion; but operators like SÄNGER, ZWEIFEL, and LEOPOLD have reason, in their repeated successes, for holding the same view. As one observer in this city has seen eleven women recover in order, it is not to be wondered at that his faith resembles that which PROF. SÄNGER claimed to hold after his first eleven cases had recovered and he had saved all of the children but the first, which was some time dead before delivery.



The general work of a large country like our own has little in it to give encouragement. Private and hospital cases, village and city operations, grouped together cannot be expected to give from 6 to 10 per cent. of mortality. We do not do it, and probably never will. When the improved operation was introduced, men were sanguine of success in anticipation, but their ardor cooled many degrees when the first 8 women died and all of the children were dead but 2, and one of these died in thirty-two hours. The dead fetuses tell the reason of failure. Labors of "thirty-six hours," "three and a half days," "a week," "nine days," and "forty-two hours," give no encouragement to the performance of "*puerperal celio-hysterotomy*." We have had 100 of these operations in the United States since October 1, 1882, and have lost 38 women and 20 children. We know why each of these women died; and death came in but few instances as a natural sequence of the mode of delivery. Obstetricians have a great deal to learn and act up to before the general mortality of our country can be reduced even to 20 per cent. It will be below this in city maternities, but private cases in little towns will keep up the death rate.

In a recent discussion before the London Obstetrical Society an opinion was expressed that the mortality under the Cesarean operation in Great Britain had been reduced to 10 per cent. "in fair cases." Now it is true that PROF. MURDOCH CAMERON, of Glasgow, did save 27 women out of 30, thereby showing the possibility of the operation in his own country; but where is the statistical evidence that even London has come down as low as 20 per cent?<sup>1</sup> No one, not even the late DR. THOMAS RADFORD, of Manchester, ever made a thorough search for the unpublished Cesarean cases of Great Britain, such as has been done through correspondence and the distribution of return-blanks in the United States. Such a statistical research has been desired for London alone, but no one has been willing to undertake the labor. These conjectural calculations have no value and teach nothing. Few men know the value of statistics as they do who collect them properly and analyze their teaching after they are chronologically arranged. It is an error to presume that because cases have never been reported they

are to be regarded as in all probability unsuccessful. We know well to the contrary, in respect to operations at home and abroad. Many an operator feels much more at home with his knife than with his pen; if asked for a report of a case, however creditable, he fails to respond; if sent a skeleton-blank to fill up, and he sees his report all mapped out and how little trouble it is to give him, he act promptly, and all the points required can be gotten. Statistics are not collected as curiosities; the cases teach how and when to operate for the saving of life, and their study has been the means, as in ovarian cystectomy, of making a highly dangerous operation one of comparatively little gravity.

3. *Symphysiotomy*. We have sometimes wished that this operation had no early history, and that it had been introduced at the time of its revival in 1866, because of the fact that this same early history lives in obstetric text-books as an obstacle to its being accepted as a legitimate and valuable alternative to the destructive operation of craniotomy in cases of moderate pelvic obstruction. It matters not that 55 Italian symphysiotomies (1886-1893) have saved 53 women and 48 children, for there stand BAUDELOCQUE and the old writers to show its fatality; its capabilities for maiming the subject; its unphilosophic character; and how little can be gained by it, in the true conjugate of the pelvis. Case after case is appealed to, to show how the forceps failed to deliver the head until after the symphysis was opened; when the same instrument availed to deliver a living fetus. Philosophic or unphilosophic, the proof stands unquestioned. PROF. MORISANI, of Naples, thought that his experiments demonstrated that this operation had a philosophic basis, and he therefore reintroduced it. The Cesarean operation in Naples had cost the lives of 25 out of 27 women, although all of the children were delivered alive. Mark the difference: the first 27 pubic sections saved 20 women, and lost 5 children. Thus much for the operation prior to the change effected by antisepsis.

They tell us in London that this operation has a higher rate of mortality than the Cesarean section, which may be true or may not be, according to the way the comparison is made. It is not correct, if we compare Leipzig with Naples; but is so, if the Cesarean work of Leipzig is compared with the symphysiotomies of Paris. It is very far from true, if we make the comparison in the United States. The Cesarean record shows a mortality of 38 per

<sup>1</sup> We have just learned that London, from Nov. 1, 1886, to Nov. 1, 1893, lost 9 women and 5 children as the result of 22 operations.

cent. There have now been 44 symphysiotomies under asepsis, with the following results: deaths of women, 5; children dead before operation, 4; died under operation, 4; died after operation, 4=12; women lost after last 28 operations, only 2; lost after the last 37 improved Cesarean, 9, or nearly  $3\frac{1}{2}$  times as many under the fairest possible comparison, *i. e.*,  $7\frac{1}{2}$  per cent. of death against  $24\frac{1}{2}$  per cent.

Symphysiotomy can only to a very limited extent be a substitute for the Cesarean operation, and properly comes between the domain of the forceps and Cesarean section, just covering the ground where craniotomy has been most abundantly resorted to, and can be employed with the least possible degree of risk. Certain German obstetricians use the forceps in such cases, and failing with this form of traction, perforate the head and reapply the instrument, claiming that their hospital records show no deaths of mothers in these cases. This is largely the practice in Great Britain now, and was at one time the universal expedient in American practice. But the introduction of symphysiotomy, and its diminishing mortality, has set the medical world to thinking upon the question of the right of the fetus *in utero* to be saved, even if in securing it the mother has to run a small additional risk. At one period the Cesarean section was the expedient to be avoided if possible, because of its death-rate, and men taxed their brains to contrive instruments that would extract a mutilated fetus through a conjugate, down to  $1\frac{1}{2}$  inches. But the fact that operating in this way, under a measurement of " $2\frac{1}{4}$  inches or less," was shown to destroy the life of one woman in five, checked the use of the cranioclast, and it has now been largely retired, since the introduction of the improved Cesarean section from Leipzig and Dresden.

Symphysiotomy is now being performed in many Continental countries, and there were 84 operations with 10 deaths in all localities in 1892, in contrast with 12 in Naples in 1891, with no death. England has made two trials, Ireland one, and Canada three. The United States stands second to Italy in successful results, and there are about as many operations now performed in Philadelphia as in Naples. When we look back four decades we see that great changes have taken place, and are now in progress, in regard to the propriety of fetal destruction; and old obstetricians, who at one time would not have hesitated to perforate the skull of a living fetus, have come to think that craniotomy should be avoided, as there are alternatives for saving the child.

## EDITORIAL COMMENTS.

*Cardiac Asthenia or Heart-exhaustion.*—Death sometimes occurs without more obvious cause than heart-failure. Such an event may be brought about as the result of inflammatory or degenerative changes in the myocardium, or possibly through involvement of the cardiac nervous apparatus. Further, a heart may be intrinsically weak, the myocardium participating in the lack of tone that affects the muscular system generally. In an address recently delivered before the Medical Classes of the University of Pennsylvania, Dr. J. M. DA COSTA<sup>1</sup> made an important contribution to this hitherto rather unelaborated subject. In this paper Dr. Da Costa deals with the feeble heart resulting from enervation or asthenia, on the one hand, and with that due to atonicity or intrinsic muscular weakness, on the other hand. In the first of these states, which usually develops in the sequence of nervous strain, the action of the heart is feeble and accelerated; the pulse is small and compressible; there is a sense of uneasiness in the precordium, but rarely actual pain; the extremities are often cold; the temperature is likely to be subnormal. The capillary circulation is poor and the skin pale, though occasionally injected or flushed. The cardiac impulse is feeble. The area of percussion-dulness is unaltered. The rhythm of the heart is rarely disturbed. The first sound is short, feeble, indistinct, lacking in volume, obscure; the second unchanged. The breathing is conspicuously unaltered, although there may be a sense of oppression. Insomnia may be complained of; depression of spirits is common. The patient is obliged to stay in bed; attempts to rise produce a sense of swooning and a vanishing pulse; or there may be actual syncope. The prognosis is favorable, though the affection pursues a protracted course. Therapeutically rest in bed is the primary indication. When the patient is able to sit up nothing does so much good as graduated shower-baths. Massage, too, may be employed, but it is more useful a little later. Swedish movements may then also be recommended, together with carefully regulated exercise, such as walking, gentle horseback-riding, or light gymnastics. The food should be most nutritious and taken as frequently and in as large quantities as the digestion will tolerate; stimulants must often be resorted to. Errors of secretion and excretion must obviously be corrected. Of drugs, strychnin, in doses of gr.  $\frac{3}{10}$ , three times a day, has proved the most useful; next in value is arsenic; of heart-tonics digitalis is the best; caffeine and cocain are useful; iron is not indicated unless anemia be present; nitroglycerin is of no avail, unless there be cardiac pain, or unless used in conjunction with digitalis. Bromids, valerian, and opium are to be reserved to meet special indications. The second form of weak heart presents, in addition to the symptoms detailed, shortness of breath, especially on exertion, and edema of the ankles and insteps. The first sound of the heart is even more indistinct and ill-defined; duplication of either sound and functional dynamic apical murmurs are more common. The influences that lead up to this condition are not at all clear; the changes if any, that take place are not evident. The prognosis is less favorable than in cases of simple cardiac asthenia. The

<sup>1</sup> "Cardiac Asthenia or Heart-exhaustion," American Journal of the Medical Sciences, April, 1894, p. 361.

treatment for the two conditions is much the same. It is probable that in some cases the two forms of cardiac weakness here discussed are associated.

**The Proper Sphere of Women's Work.**—In a recent address Sir Dyce Duckworth claims that the experience arising from a long period of practice in the medical profession renders its owner peculiarly well-fitted to form an opinion on the proper sphere of women's work. Briefly, he takes up the position that the fixed sexual differences between women and men make it impossible for the former to expose themselves to the training and influences suitable to the latter, without materially and disastrously affecting the welfare and happiness of the human race. Speaking as a physician, he maintains that the physical strength and energy of all but exceptional married women find ample scope for their exercise in the household and family work, which can be efficiently performed by none other than themselves, and asks where are the "wonderful women" who, after properly discharging those duties, can yet find time and energy to compete with men in the pursuit of business or public work? To those who are without such duties or find them insufficient, he would advise the adoption of interesting and suitable outside work, but would avoid politics, public work of most descriptions, the professions, and above all that of medicine, as being unsuited to the very nature of the female sex.

This opinion of a well-qualified observer is, however, contradicted by the opinion of thousands of better-qualified American physicians. We say better-qualified, meaning thereby more experienced in the special matter. Thousands of women as well as men find "the welfare and happiness of the human race" affected for good by the work of women in medicine. Entirely too much has been made of the old bogey of the "fixed sexual differences between women and men." It is hardly good logic or good morals to put a person in a dark room for years and then blame the person for paleness and weakness. The position of women has outfitted them with some lack of "physical strength and energy," but they should hardly be blamed for it, and the girls even in England seem to be able to outdo the boys in the contests for university honors, and with us there are many excellent and successful women physicians.

**Attenuated Quackery.**—We have received from the — Surgical Instrument Co., a letter, a pamphlet, and an instrument. The letter asks us to notice the instrument, which is "not advertised as a cure-all," but "comes nearer to being one than any of the thousand things that are so advertised." The pamphlet is a reprint of Dr. —'s paper in *The Medical* —, describing the instrument, together with price-list of instrument and medicine to be used with it.

The instrument ("patent applied for") appears to be a good device for applying well-known principles long in use and often written of by well-known physicians of Philadelphia and elsewhere. The method of its advertisers comes so near to quackery that it takes an expert casuist to draw the line.

We do not suppose that Dr. — is responsible for the doings of the — Surgical Instrument Co., but we would advise him in the interest of his good name and

his deservedly high reputation to insist on a modification of the pamphlets, circulars, etc., furnished with his instruments.

If the code-revisers have their way, and patenting of medical and surgical instruments and remedies is allowed, it will be a sorry day for the medical profession. The narrow line between the doings of the — Instrument Co. and quackery would soon be overstepped, and the result would be degrading to all concerned.

**Protracted Anuria.**—We have to add another case to those already recorded (see *THE NEWS*, November 18, 1893, p. 582) in which functions ordinarily viewed as vital have remained for many days in abeyance. KAEFER (*Berliner klinische Wochenschr.*, 1894, No. 13, p. 319) reports the case of a man, sixty-eight years old, and previously in excellent health, who came under observation after not having passed urine for six days. He suffered no discomfort and, but for the consciousness of the failure of the renal function, would have had no concern. It was only after having been struck by the fact that he had passed no urine for more than twenty-four hours that the man found, on attempting to do so, that he was unable to evacuate a drop. There was a little abdominal pain, but there had been no attack of colic. On the eighth day, however, such an attack did occur, after which for a time the secretion of urine was resumed; the fluid was found to contain pus-corpuscles and epithelial cells, but no urates. A short time later a second attack of anuria occurred, lasting for five days, when death took place amid the manifestations of pulmonary edema. The post-mortem examination disclosed the presence of obstruction of both ureters by calculi, with beginning hydronephrosis on the left side.

## SOCIETY PROCEEDINGS.

### PHILADELPHIA ACADEMY OF SURGERY.

Meeting March 5, 1894.

THE PRESIDENT, DR. WILLIAM HUNT, IN THE CHAIR.

(Continued from page 473.)

**CASE II. Large strangulated inguinal hernia; sloughing intestine; radical operation, with removal of testicle; recovery.**—Jacob B., aged forty-three years, was brought to me by his physician, Dr. James Robinson, on March 6, 1893, and admitted to the Polyclinic Hospital. He stated that he had had a large tumor in the left scrotum from childhood, and could not remember when it had originated. By manipulation at times he thought that it had become somewhat smaller. It never had given him any annoyance.

On March 1st, while in apparently perfect health, he drank a quantity of ice-water, which induced vomiting, during which he felt a sensation of something in the left inguinal region "giving way." From this time he continued to vomit everything taken, and to experience extreme nausea. The bowels also ceased to move. The tumor became more and more painful and swollen. He continued in this condition five days, and then summoned Dr. Robinson, who applied taxis for thirty minutes without making any impression on the mass. On the sixth day he walked a number of squares to the hospital.



Upon admission I found a tumor the size of a large cocoanut occupying the left scrotum and inguinal canal. The testicle was below the general mass, and could be outlined as distinct from it. The whole tumor was very tender and somewhat edematous. Deep fluctuation was marked. No percussion-resonance or succussion could be elicited. A very resisting mass occupied the inguinal canal. The bowels had not moved for six days. There was constant fecal vomiting. The general condition was fair.

A diagnosis of chronic hernia of the omentum, with acute entanglement of a portion of the intestine, together with hydrocele, of the hernial sac, was made, and immediate operation insisted upon.

After preliminary cleansing and anesthesia, without waiting to make prolonged taxis, which was clearly prohibited by the long duration of the strangulation (as the subsequent history clearly demonstrated), a five-inch incision was made in the line of the inguinal canal and scrotum. The cellular tissues were edematous and had some odor of decomposition. The sac was exceedingly thick and highly discolored, as if about to break down. It was freely incised, and half a pint of dark, turbid fluid of putrefactive odor escaped. The sac contained a large mass of hypertrophied omentum of the character so frequently found in omental hernias; it was adherent by firmly organized attachments to most of the sac-wall. It was purple in color, intensely congested, and very edematous. The whole was stripped from its adhesions and carefully spread out, whereupon a portion of small intestine came into view that had been entirely wrapped up in the folds of the omentum. This again illustrates the extreme care that is always so necessary in handling the contents of a hernia; for it would have been exceedingly easy to have ligated off the omentum and bowel together in this case, and to have returned the stump to the abdomen, thus insuring a fatal issue. A finger passed into the inguinal canal discovered a very tight constriction at the internal ring, which was freely divided upward. The omentum could now be pulled down with ease until entirely healthy portions appeared, when it was ligated off in two bundles by transfixion, and the stump returned to the cavity and fixed immediately behind the internal ring by bringing both of the ligature-ends through different portions of the muscular wall, and then tying them down, as suggested by MacEwen for making a protecting pad of the sac in his radical operation. The prolapsed intestine could now be freely inspected.

Upon the free margin was a gangrenous spot, one-half inch in diameter, and which appeared to be in imminent danger of rupture. The remaining portions of intestine were almost black from venous stasis, but were not actually sloughing. The sloughing portion was buried under healthy peritoneum by means of Lembert sutures, buckled into the lumen of the bowel, as it were, just as if perforation had taken place. Fortunately, this procedure did not too much diminish the lumen of the bowel; and by the time that it was completed all excepting the sloughing portions of the intestine had, under hot irrigation, resumed the beautiful pink tint of returning circulation. The bowel was then returned to the abdominal cavity.

After a very difficult dissection the sac was freed of its

attachments to the scrotal, testicular, and cordal tissues, and ligated off in two sections at the internal ring. The stump of omentum was next fixed behind the ring as described. Attention was now turned to the testicle and cord. These structures were edematous, had the general foul odor of all the surroundings, and, in spite of careful separation, had been considerably injured by the dissection and other manipulations. It was considered that leaving such structures in a wound already gravely threatened with suppuration, if not sloughing, was an unjustifiable risk to which to subject the patient, as the testicle, under these circumstances, was likely to break down and slough, with accompanying dissecting suppuration, and especially was it desirable to secure for this hard-working man a radical cure, if possible. Hence the cord was ligated at the internal ring most carefully by a transfixing double ligature of silk, and also returned to the abdomen. This permitted a careful and complete closure of the internal ring and the whole inguinal canal by buried silkworm-gut sutures placed about three-eighths of an inch apart. A small rubber drain was placed in the lower portion of the scrotal wound, but none in the upper portion, and the skin-margins were approximated by silk sutures.

Excepting a small amount of suppuration in the scrotum, the entire wound healed by primary intent. The drain was removed on the third day. The bowels moved within twenty-four hours spontaneously, and naturally each day thereafter. There were some small clots of blood in the first stool, but none subsequently. The man was given absolutely nothing by the mouth for thirty hours subsequently to the operation; but rectal enemata of warm water were frequently administered, and this prevented thirst. Then liquid diet was started and full diet gradually resumed. The man was kept in bed three weeks, and resumed his duties as an hotel-waiter in five weeks. He was instructed not to wear a truss.

I last saw this man on November 2, 1893. There was no sign of return of the hernia. The cicatrix was sound. He has been working hard at his occupation ever since the operation, has not worn a support, and expressed himself as stronger and better in every way than he had been for years.

*CASE III. Strangulated congenital inguinal hernia; radical operation, with removal of undescended testicle; recovery.*—Irvin B., aged nineteen years, was admitted to the Polyclinic Hospital, May 9, 1893. He had enjoyed excellent health all his life, but had always had a "come-and-go" tumor in the right scrotum, and had never been able to distinguish a testicle upon that side. The opposite testicle was large and firm. The tumor could always be reduced with ease, and likewise disappeared whenever he laid down. It was painless. He had never worn a truss.

On the morning of May 8th, upon slight straining while at his work of tile-setting, he felt a painful and unusual sensation in the right side of the scrotum, and went home, nauseated and in great suffering. He then discovered that a tumor much larger than usual was present, and that it was tender and irreducible. The bowels moved a few hours subsequently.

On May 9th he applied to me for relief, and I insisted upon his entering a hospital. No movement of the

bowels had taken place except shortly after strangulation. There was much nausea and anorexia, but no actual vomiting had taken place. The belly was somewhat distended, but the general condition was good. In the right inguinal region was a tumor of considerable proportions and very sensitive. Fluctuation was very marked in the dependent portion of the scrotum. There was no succussion and no percussion-resonance. The inguinal canal was tense and occupied by a solid mass. The testicle could not be defined.

Upon admission the man was given a bath, and, consent to removal of the undescended testicle having been secured, immediately etherized. Taxis was then made for five minutes, and failed. Immediately upon failure of taxis a long incision was made over the inguinal canal and tumor and carried down through the tissues until a thin-walled sac was freely exposed. This was also incised, and four ounces of brownish serum escaped, disclosing a web of congested omentum and about four inches of small intestine. The omentum was inky-black, the intestine not quite so much injected. A very small and tight constriction was found at the internal ring. This was divided with a Cooper hernia-knife with considerable difficulty, owing to the high location of the ring and to the intense constriction—bowel as well as sac overlapping on all sides. A small rent in the serous covering of the bowel resulted, but was immediately repaired with half a dozen Lembert sutures. I have since learned to cut the ring from above in all difficult cases, and thus to avoid the risk of wounding the bowel or other structures passing through the ring. As a division of the ring and roof of the canal from above is now an established procedure when making radical cures for hernia in this situation, division of the ring from below may go entirely out of fashion, excepting in desperate cases or in the very aged or feeble, in whom radical operations must be ruled out, owing to the increased danger to life inseparable from prolonged operative manipulation.

As soon as the constriction was relieved the bowel and omentum at once resumed their normal color and circulation. The omentum was ligated off in portions and the stump returned to the abdominal cavity. More intestine was then pulled down, as usual, but no further constriction was found, and it was also returned to the cavity.

In the sac-wall at the position of the external abdominal ring was found an undeveloped, undescended testicle, about half an inch long and three-eighths of an inch wide. This was dissected out with a portion of the sac, the pedicle securely ligated, the testicle cut away, and the stump returned to the peritoneal cavity. It could have been reduced with the sac, but could not have been returned to the scrotum with a cord manufactured out of a portion of the sac-wall including the testicular vessels and vas deferens. When I considered the uselessness of the organ in its undeveloped condition, its well-known propensity to degenerations and other diseased conditions, and, moreover, as I had secured the man's consent to its removal, and could make so much better a radical operation with no structure traversing the rings or inguinal canal, it appeared more than justifiable to sacrifice the organ, especially as its fellow was in perfect condition. The sac (containing the pedicle of the cord)

was next dissected free from its surroundings up to and a little within the internal ring. Here it was puckered up after the method of Macewen with a thread and needle and fixed just behind the internal ring, to act as a pad to protect that region from the possible return of a portion of gut. The internal ring and the inguinal canal were then completely obliterated by silk sutures and the skin likewise sutured by silkworm-gut. No drainage was employed. The bowels were moved by fractional doses of calomel and podophyllin within twenty-four hours, and in three days he was eating semi-solid diet. The wound healed throughout by primary union. He was kept upon his back for three weeks and resumed his occupation in five weeks. No truss was prescribed.

I have seen this patient recently—almost one year since the operation—and found the cicatrix sound, with no sign of return of hernia.

DR. JOHN H. PACKARD: I wish simply to refer to one practical point in the treatment of these cases, and that is the use of swimming drawers to hold the dressings in place. It is difficult to apply a bandage that will keep its place perfectly, and I have found this method to answer an exceedingly good purpose after various operations in the pelvic region.

DR. JOHN B. DEEVER: In this connection I should like to state that Bassini reported one year ago 560 operations, with 15 relapses and no deaths.

There are only two points which I wish to mention. One is the kangaroo tendon now used by most surgeons. The second is enveloping in a plaster-of-Paris bandage. A great deal of the perfect asepsis is due to the latter.

DR. ORVILLE HORWITZ exhibited an improved Wheelhouse staff.

## CORRESPONDENCE.

### LONDON LETTER.

*Ventro-fixation of the Uterus—The Bruits of Chlorosis—  
—A Method of Examining the Bladder in  
Patients with Enlarged Prostate—  
Raynaud's Disease—Sulphur  
as an Antiseptic.*

DR. SINCLAIR reports twelve cases of ventro-fixation of the uterus, or hysteropexy, as it is called in France, for the relief of retroflexion. They were all successful, and in two cases pregnancy afterward occurred without untoward result. Sinclair recommends that the fundus of the uterus should be seized exactly in the middle line by a small volsella, by which it can be drawn up and held in any position, which proceeding he thinks is much better than the use of a lever inside. The vesico-uterine fold is first obliterated by two rows of cat-gut sutures passed through the parietal peritoneum and the peritoneum of the uterus. Higher up, near the origin of the broad ligament, the uterus is fixed to the parietal peritoneum, the permanent sutures entering the uterine tissue but only passing through the peritoneum on the parietal side. Finally, two silk sutures are passed through the whole thickness of the parietes, through a portion of the uterine tissue on the anterior surface of the uterus and out again through the parietes. (These

are only temporary supports and are removed on the third day.) The abdominal wound is closed by silk sutures in the ordinary way. There were no deaths after the operation and no cause for anxiety. The most disagreeable results have been the occurrence of sinuses produced by the suturing material and of ventral hernia in some cases, especially those in which pregnancy took place. The operation seemed in every case to give great relief from the pain and discomfort for the removal of which it was performed, and Dr. Sinclair appears to think that (a) the danger of the operation is almost *une quantité négligeable*; (b) when efficiently performed it is a permanent cure for the troubles caused by chronic retroflexion with adhesions or inflammatory affections of the tubes and ovaries; (c) no unusual phenomena are observable during pregnancy or parturition occurring after the operation; (d) bladder-troubles are quite the exception; (e) ventral hernia occurs at the site of the cicatrix in a certain number of cases; (f) after making full allowance for the element of risk and the incidental drawbacks, the operation of ventro-fixation is not only justifiable, but is indicated in a certain limited class of cases of retroflexion of the uterus with complications.

Dr. Frederick Coley draws attention again to the venerable questions, which have been discussed so often and from so many points of view, as to the meaning of the vascular bruits heard in cases of chlorosis. What is of importance, however, is that he is able to assert that, contrary to what is so often taught, a bruit is not at all uncommonly discoverable in chlorosis not only at the apex of the heart but extending outward to the axilla and to the angle of the scapula. Among 400 consecutive cases of chlorosis he found 278 which presented a bruit at the apex and 123 having a bruit at the angle of the scapula also. It is well to remember that the bruits connected with chlorosis alter much with change of position, and that while one at the apex and in the axilla can often be discovered only when the patient is lying down, a bruit at the angle of the scapula is never so heard unless it is also audible in the upright position. No doubt these abnormal sounds are the result of dilatation of the left ventricle; and when questions arise as to whether a given bruit is due to organic disease or to the dilatation common in chlorosis, it is worth remembering that the two conditions may exist together and that under such circumstances it is most important not to overlook the chlorosis, for the intercurrent of this condition, itself sufficient to cause dilatation, is the thing of all others most likely to lead to failure of compensation in a case of organic heart-disease.

Mr. Hurry Fenwick has described to the Medical Society a method of examining the bladder which he finds useful in those very troublesome cases in which symptoms of stone exist along with a prostate so large as to prevent a proper investigation of the pouch behind it. Of course, in such cases, a stone may chance to be felt at once, but it is extremely difficult to assure oneself that no stone exists when it shows no sign. Mr. Hurry Fenwick fills the bladder, punctures with a trocar above the pubes, and then introducing a sound or an electric cystoscope through the canula is able to thoroughly investigate this pouch which has so often been a cause of embarrassment. If on examination anything should be found making it desirable to open the bladder

more completely, it is easy to make the canula a guide for the incision, and Mr. Fenwick showed a calculus which he had in this way discovered and removed from its attachment to the posterior surface of the prostate; removing at the same time the projecting portion of the prostate itself.

After giving the clinical history of a case of Raynaud's disease, Dr. Hale White discusses its pathology. It is clear at once that the condition which we see is due to a constriction of the minute vessels of the affected part. The larger vessels are found beating quite near the disease, and after death are found free from embolism or disease. In discussing its origin, then, we may assume that the local syncope and the local asphyxia, which in some cases lead to local death, are the result of spasm of the minute vessels. That drives one at once to the nervous system as the cause, or, at any rate, the route by which the spasm is set up. And, as a fact, peripheral neuritis has in a certain number of cases been shown to exist. It seems, however, by no means certain that this neuritis was not set up by the changes induced by the vascular derangement, and that it was not in fact the result rather than the cause of the disease. Then we must remember that vasomotor changes can be produced by diseases of other portions of the nervous system; in fact, spasm is more suggestive of either central disease or reflex irritation than it is of peripheral neuritis, and there is the suggestive fact that many of the cases of Raynaud's disease occur in hysteric and insane subjects. We should then look upon it as a symptom indicative of disease affecting some portion of the nervous vasomotor system, shading off, quite imperceptibly, into the cold dead fingers to which we are all liable. This puts it into alliance with erythromelalgia, a vasomotor neurosis in which the vessels dilate instead of contracting, and also with cases of urticaria and local mottlings. This view warns us not to expect to find an organic cause for the disease. We can no more hope always to find an organic explanation for paroxysmal vasomotor attacks leading to blueness of the fingers, than always to find an organic explanation for paroxysmal epileptic fits.

Sulphur is no doubt in some surroundings a very harmless substance. It evidently, however, when in combination with oxygen or hydrogen, produces compounds which act strongly as bactericides, and Mr. Arbuthnot Lane has during recent months used it largely at Guy's Hospital as an antiseptic. Innocuous as it may seem, and as it actually is to skin and mucous membrane, it produces, in contact with recently incised tissues, compounds which are powerfully caustic in their action, and must, therefore, be used with caution. Perhaps the best plan is to make it into an emulsion with glycerin, by means of which its action is rendered less violent and more generally diffused. If sulphur is used in any quantity it must be removed in a day or two, and the wound irrigated. Cases of tuberculous disease in which there is doubt whether the whole of the infected tissue has been removed, and cases of injury in which dirt has been deeply imbedded, would seem to be the typical cases for its employment, in both of which, from its slow conversion into substances fatal to organisms, it acts as a continuously acting reservoir of bactericidal material. It may also be used as an application to lupus, carcinomatous or sarcomatous ulcerations, or in cases of ulcerative stomatitis.



The fact that during its presence in a wound  $H_2S$  is formed must be borne in mind, otherwise the smell might give rise to a false impression that septic decomposition was in progress.

### ENUCLEATION OF THE UTERUS.

To the Editor of THE MEDICAL NEWS,

SIR: In the issue of February 10, 1894, of your esteemed journal, which I saw only yesterday, I find an article by Dr. Robert Reyburn, on "Vaginal Hysterectomy for Carcinoma of the Uterus performed by Enucleation without Hemorrhage," which reminds me forcibly of a discussion on the possibility of this operation at a meeting of the New York Obstetrical Society held October 17, 1876 (*Amer. Journ. Obstet.*, vol. x, p. 106). Noeggerath reported a case of vaginal removal of a carcinomatous uterus with a fatal result, and during the discussion the following remarks were made: "Dr. Mundé referred to a case reported by Dr. Alexander Patterson, of Glasgow (*Glasgow Med. Journ.*, 1876), of the successful removal of the whole prolapsed uterus by an operation very similar to that performed by Dr. Noeggerath. Bearing on a case of his own, reported in the *American Journal of Obstetrics* for August, 1872, in which the whole uterus (at least what was left of it, fundus and body) sloughed out after the operation of curetting and applying chromic acid for cancer of the cervix, Dr. Mundé asked whether, in case of relaxation of the connective tissue attachments between the uterus and the peritoneum it might not be possible to peel the uterus out of its peritoneal envelop without opening the peritoneal cavity. [Italics new.] Dr. Noeggerath replied that, in certain abnormally relaxed cases it might be possible; in the normal or ordinary condition never. In this opinion Drs. Peaslee and Thomas concurred."

It will thus be seen that the possibility of the operation performed by Dr. Reyburn had occurred to me as long as eighteen years ago. But experimenting on the dead uterus showed me that such an enucleation of the body of the uterus from its peritoneal envelop was impossible, and I have placed myself on record elsewhere in regard to this subject. (See an article on "Obscure Pelvic Abscess in Women" in Seguin's *Archives of Medicine*, December, 1880, page 3 of the article.) There, speaking of the existence of cellular tissue between the pelvic organs, I said:

"Spiegelberg says in a clinical lecture that the whole cervical portion of the uterus—that is, the portion of the cervix situated between the reflection of the vagina and the internal os—on all sides except in front, is surrounded by abundant loose connective tissue, and that the retro-uterine peritoneum is not closely attached to the uterus. Of this I have had occasion to satisfy myself while testing the possibility of enucleating a uterus from its peritoneal envelop through the vagina, as would be indicated in carcinoma of that organ. I found that the peritoneum could very easily be detached from the uterus up to the os internum, but that above it was impossible to separate it except by actual dissection with the knife. In front this detachment was by far less easy than behind and at the sides, owing to the small amount and density of the connective tissue, which, at a spot corresponding to the os internum, is interspersed with muscular fibers and

assumes the character of a ligament, whereby the uterus and the bladder are so closely connected as to be almost inseparable." [Italics new.]

Unless Dr. Reyburn's case was an unusually favorable one for enucleation, owing to the senile, relaxed condition of the peritoneal envelop of the body and fundus of the uterus (as the Doctor himself admits), I can hardly think it possible to peel off the peritoneum as easily as he describes it to have been done. Indeed, I am inclined to look upon the operation more as a dissecting away of the uterine body from its peritoneal covering than as an enucleation or peeling out. In this respect the operation would not differ so much from the step-by-step excision of the carcinomatous uterus carried out years ago by J. Marion Sims, when often but a shell of the organ was left. I am sure that in a younger woman the division of the vessels leading to the uterus would not have been as bloodless as in Dr. Reyburn's case.

I shall look with interest for further reports of this operation, which, now that it comes from an authentic source, assumes a far more credible aspect than when it was reported from a doubtful and irregular quarter.

Since writing the foregoing my attention has been called to a second article by Dr. Reyburn in THE MEDICAL NEWS for February 24th, in which he discusses the question of the priority of the idea and supposed execution of "enucleating" the uterus. It is not my intention to claim any such priority, although I was not aware that any operator had ever thus actually enucleated the uterus. My object, indeed, is to question the possibility of doing so, on account of the naturally too firm adhesion of the peritoneum to the body of the organ. Dr. Reyburn quotes Langenbeck's case (1813) as the first detailed one of enucleation. But a reference to Langenbeck's description, as quoted by Dr. Reyburn, shows that that operator left a slice of the fundus uteri attached to the peritoneum. Besides, the uterus was prolapsed, that being the reason for its removal, and probably the peritoneal attachment was much loosened. Such loosening does not occur with the uterus *in situ*. The uncertainty of Langenbeck's operation is shown by his thinking he had removed the ovaries when the post-mortem showed that he had not done so. Malgaigne's account of enucleation is apparently theoretic only, as is also that of Récamier. Sauter, of Constance, who in 1822 first removed the uterus *per vaginam*, did so in a manner similar to our present method of vaginal hysterectomy. While abroad in 1868 I accidentally came across an illustrated graduation-thesis by a Dr. von Siebold, a nephew of the celebrated obstetrician Prof. von Siebold, printed in Göttingen in 1827, describing in all its details Sauter's operation. I valued this thesis very highly, both for its historic interest and for the excellent engravings of the operation. But I unfortunately lent it some years ago to a friend whose identity I have forgotten, and it was never returned.

I will say in conclusion that I neither question nor criticise Dr. Reyburn's operation. I merely wish to call attention to the fact that eighteen years ago I had the same idea and found its execution impossible on the dead uterus.

PAUL F. MUNDÉ.

20 WEST FORTY-FIFTH STREET, NEW YORK,  
April 7, 1894.

## A CASE OF ARRESTED DEVELOPMENT.

To the Editor of THE MEDICAL NEWS,

SIR: I send you for publication the following statement descriptive of what I consider a very remarkable case of arrested development. The examination was made June 25, 1883. I have not seen the subject for six years, but learn that he is now living in an adjoining county. I was not able to learn at the time of taking the measurements whether or not the mother was subjected to any unusual mental strain during pregnancy.

L. F., of Balltown, Mo., aged fourteen years and six months; weight sixty-eight pounds, height three feet two inches; wore a No. 7 hat. The boy was bright and intelligent. His body was of usual size for his age, and was well developed. The humerus of each side was normal. On the right side, the ulna was three inches and the radius one inch in length. There was an appearance of intra-uterine amputation at the radio-carpal articulation. On the left side, the radius and ulna were three inches in length. The wrist contained four or five imperfectly developed carpal bones. There were two metacarpal bones, viz., those of the thumb and index finger. There were no phalanges for the thumb, but there was a well-developed nail on the end of the metacarpal bone. There was no osseous but only cartilaginous development of the first phalanx of the index finger, which was without a nail. The left femur was seven inches in length; the right femur five and a half inches in length, and was set at a right angle to the pelvis, the knee being on a level with the acetabulum. The tibia and fibula in both legs were well developed, except that on the right side they were about one and a half inches shorter than on the left. The left foot was well developed except the great toe, which was abnormally large. The tarsal bones of the right foot were imperfectly developed, with talipes varus.

The left testicle was down and well developed. The right tunica vaginalis seemed to be perfect, but the testicle was wanting. The child was the second in a family of nine children. The rest are all of average size and well developed.

Respectfully,

J. ROBERT BUCHANAN.

NEVADA, Mo.

## A PLEA FOR CHAIRS OF THE HISTORY OF MEDICINE IN MEDICAL COLLEGES.

To the Editor of THE MEDICAL NEWS,

SIR: In THE MEDICAL NEWS for March 24, p. 336, under the heading "Medical Progress in Cleveland, Ohio," your correspondent says:

"Professor Kelly presented to the [Cleveland Medical] Society a rare old book—a copy of the work on *Anatomy*, published by Vesalius in 1514, and also one published by Tulf, in Amsterdam, in 1716, entitled *The Century of Observations*."

Vesalius, the Luther of anatomy, was born in the year 1514, at Brussels. His monumental work: *De humani corporis fabrica libri septem*, was published, not by himself but by Oporinus, of Basel, in 1543. Doubtless this is the work of which your correspondent speaks. This, however, was not the earliest of the books of Vesalius, for, in 1537, his *Paraphrasis in nonum librum Rhasae*

*ad Almansorem*, was issued at Basel. His first anatomic book, an epitome of human anatomy, was published in the same city in 1542. It is upon the first work mentioned that the fame of this great man securely rests.

Now a word as to Tulp or Tulpus, not Tulf. Born at Amsterdam in 1593, Tulpus was a surgeon of great renown. His face is the most prominent figure in Rembrandt's celebrated painting, *The Lesson in Anatomy*, which hangs in the Hague Gallery. The only one of his books in my library is the *Observationes Medicae*, published at Amsterdam, by Daniel Elzevir, in 1672. Tulpus died in 1674.

It is unnecessary to state that I have derived much pleasure, as well as some profit, from the works of Vesalius, being the fortunate possessor of two editions of his great *Anatomy*: Basel, 1543, and Leyden, 1725.

I may also be pardoned for expressing the opinion that it is time for the medical schools of America to establish chairs of the History of Medicine, as has been done by many leading schools of the Continent.

Very truly,

JAMES MOORES BALL,

Professor of Ophthalmology and Lecturer on the History of Medicine in Keokuk Medical College, Keokuk, Iowa.

## A MONSTROSITY.

To the Editor of THE MEDICAL NEWS,

SIR: On March 19, 1894, I was called to attend Mrs. J. L., in her sixth confinement. Although insane for the last seven years, she has borne three children, two bright, intelligent ones and a third, the subject of this paper. Her physical health has been good; but about two and a half years ago she had to be confined in an asylum until about one year since, when she returned in her usual harmless, insane condition.

This last confinement had just terminated when I arrived at her home and was told by the neighbor in attendance that it had been a breech-delivery. On inspection, the child appeared to be at full term, but was dead, as existence was an impossibility with the anomalies that presented themselves.

The head, shoulders, and arms were perfectly developed. The chest was in shape a sharp ovoid, pointing antero-posteriorly. There was complete ectopia of the heart and all the abdominal and pelvic viscera, the cord, four inches in length, having the placenta attached throughout its entire length. Evidently all the uterine contents had been expelled *en masse*. No semblance of sexual organs could be discovered, there being an absence of the pubic arch. What should have been internal organs were well developed and distinguishable, but the rectum terminated in a blind pouch. From the point of the missing pubic arch the perineum was perfectly smooth, there being no vestige of an anus.

A spina bifida, including the two lower lumbar vertebrae and a portion of the sacrum, gave rise to a tumor as large as an ordinary orange. Complete double equinovarus of the feet existed, and the legs were crossed tailor-fashion and could scarcely be made to assume any other position. The big toe projected from the left foot at a right angle like the thumb of a hand. The entire weight was six pounds.

WILLIAM W. PENNELL.

FREDERICKTOWN, OHIO.

## A SYMPHYSIOTOMY.

To the Editor of THE MEDICAL NEWS,

SIR: The following case of symphysiotomy has so far not been reported:

I operated on Mrs. M., August 28, 1893, after the recognized method, and extracted a fetus, alive, weighing fifteen and one-half pounds. The child lived eight days, dying, not as a result of the operation, but from severe diarrhea, from which, indeed, all of the inmates of the house suffered at the same time.

The great distention of the parts beneath the symphysis caused a laceration, beginning at the left side of the meatus urinarius, extending backward along that side of the urethra, and involving the bladder. This laceration was subsequently repaired. The patient is now well.

I. WOTHERSPOON.

SEATTLE, WASHINGTON.

## NEWS ITEMS.

*The International Sanitary Conference*, recently held at Paris, after having been in session for nine weeks, formulated conclusions which were formally agreed to by all of the participants except the representatives of Turkey and the United States. The Conference divided its labors into three branches. The first had to do with the whole question of the Mecca pilgrimage both from the East and from the Mediterranean, and it was dealt with by a "Red Sea Committee." The idea was to carry out such regulations at ports of departure, during the voyage, and on arrival in the Hedjaz, as would prevent any cholera reaching the Holy Places either from India and the Dutch Indies on the one hand, or from the basin of the Mediterranean whenever that disease prevailed in the west. Great Britain and India have willingly consented to have all intending pilgrims subjected to medical examination before starting; to maintain them under medical supervision between Indian ports and Aden, and to report the result at the latter place, where any infected vessel would be adequately dealt with; to insist on having a properly qualified medical man on board every pilgrim vessel; to increase the space allotted to pilgrims on board; and to enforce very substantial fines for any breach of a large number of regulations affecting the voyage. In return for this they claimed a diminution of restrictions in the Red Sea, and they condemned in no very measured terms the cruel and evil results following the quarantine imposed on pilgrims in the island of Camaran before disembarking at Jeddah. The result has been a decision of the Conference calling upon the Turkish Government to discontinue putting healthy vessels into quarantine, and admitting for the future only a short detention of forty-eight hours in order to examine the pilgrims and disinfect their personal effects. The Turkish Government has also undertaken to erect decent buildings on the island for the reception of the pilgrims, and also to arrange for an improved water-supply and better sanitary arrangements. Much the same arrangements have been made as to pilgrims reaching the Red Sea from the north, and the Turkish delegates have also assured the Conference that measures are now being adopted to improve the sanitary conditions of Jeddah and Mecca.

The second branch of the work of the Conference was prepared by a committee called the "Persian Gulf Committee." It proposed to transfer from Bassorah on the Euphrates to Fao at the mouth of the Shat-el-Arab, the principal quarantine-station for the Gulf, and to carry out there a system of inspection and detention under observation much on the lines of the arrangements laid down in the Dresden Convention. It was also intended to establish at different points along the Gulf a series of sanitary stations. The British and Indian delegates all along declared that this was not a route by which cholera was so likely to travel as to justify a serious hindrance to commerce; and since the scheme involved in addition a number of territorial and other allied questions, the British delegation have refused to accept the decisions of the Conference on this subject.

The third branch of the work was undertaken by a committee of "ways and means." It has decided what the Turkish Government is bound to do in return for the almost endless taxes it imposes on the pilgrims; it has set out how the Council of Constantinople should spend some of its receipts; and, above all, it has come to an almost unanimous decision that a new body is to be formed within the Council of Constantinople to administer and carry out the terms of the Convention. No country is to be represented in this body unless it has accepted the terms of the Venice, Dresden, and Paris Conventions, and thus there will be elimination of those representatives who are delegated by quarantining countries. A period of one year will elapse before the final ratification of the agreement.—*Lancet*, April 7, 1894, p. 898.

*Surgical Section of the College of Physicians.*—At the next meeting of the Section, to be held on Friday, May 11th, at 8 P.M., in addition to other interesting material, Dr. J. William White will read an elaborate essay on "Appendicitis." Dr. Robert F. Weir, of New York, will open the surgical and Dr. William Pepper the medical discussion, which will be continued by Drs. William W. Keen, John H. Musser, Thomas G. Morton, S. Solis-Cohen, John B. Roberts, De Forest Willard, Harrison Allen, and others.

*Cholera* has again made its appearance in various parts of Europe. On April 25th there were reported at Lisbon 225 fresh cases of mild type; on April 26th, 87 cases; on April 28th, 128 cases; on April 29th, 45 cases; on April 30th, 26 cases. The disease prevails also in East Galicia, in parts of Russia, and in France. It is announced that the Russian Government has erected five sanitary stations on the Austrian frontier, owing to the prevalence of cholera.

*The Medical Profession in Russia.*—There are said to be 18,334 practising physicians throughout the Russian empire, among a population of 110,000,000, about 1:6000. In the cities and larger towns the proportion is about 1:2700, while in the smaller towns and villages it is about 1:30,000, and in some of the more remote provinces it is said to be not more than 1:120,000 inhabitants.

*Dr. Ludwig Pfeiffer*, of Munich, has been made Professor of Hygiene in the University of Rostock.